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Psychological Review

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VOL. XXIV. No. 1 January, 1917

THE PSYCHOLOGICAL REVIEW

THE RELATION OF INTELLIGENCE TO SOCIAL STATUS

BY JAMES W. BRIDGES AND LILLIAN E. COLER

Ohio State University

HISTORICAL

Among the many reports of investigations with the Binet-Simon scale incidental references to the influence of social status on intelligence are occasionally found; but heretofore the study of this extremely important and interesting topic has been relatively neglected, although Binet with his usual acumen did not overlook the problem.

In 1910 Decroly and Degand[1] tested forty-five children of both sexes in a private school at Brussels. They found that none of the children tested were below age, nine were at age, and the rest from one to three years above the level of their age. These results were very significant, since the Binet-Simon scale is theoretically supposed to rate equal numbers retarded and advanced with the mode and the average at age.

Decroly's and Degand's results were carefully studied by Binet.[2] He thought the best explanation of the difference between his results and theirs was found in the fact that the Belgian children came from a private school in Brussels and represented children of the well-to-do and largely professional class while the Paris children were from a rather poor section

[1] Decroly, O., et Degand, Mlle. J., 'La Mesure de l'intelligence chez des enfants normaux d'après les tests de MM. Binet et Simon,' *Arch. de psychol.*, 1910, 9, 81–108.

[2] Binet, A., et Simon, Th., 'Nouvelles recherches sur la Mesure du Niveau Intellectual chez les enfants d'école,' *L'Année Psychol.*, 1911, 17, 145–201.

of the city. The instruction in the Belgian school was also more individual for the classes were very small.

In analyzing the Brussels results by the individual tests, Binet found that the children from the better social class scored higher in tests involving thought in the higher sense—apprehension, criticism, comparison, etc. They also scored higher than the less favored children in the tests which put a premium on linguistic readiness—such as the description of pictures, abstract definitions, comparison of objects, absurdities, and giving words for three minutes. Binet concluded that social status must be closely correlated with mentality and reckoned from the results in the two schools an average difference of about one and one half years between children of the better and poorer social classes. He does not take into account, however, that this difference may vary with different chronological ages.

Binet, moreover, criticized[1] the work of Katharine Johnston, who had examined two hundred pupils of the Sheffield schools in England, because she had drawn her subjects from at least three distinct social groups and had not kept these groups separate in her averages.

Another study which seemed to confirm Decroly's and Degand's results was the testing done by M. Morlé[2] in a school in a poor part of Paris and compared with the results from a school situated in a wealthy section. The study was on a rather small scale as only thirty children were taken, at random, from each school. The results were as follows:

	Retarded		At Age	Advanced	
	2 Yr.	1 Yr.		1 Yr.	2 Yr.
Unfavored school	1	11	13	4	1
Favored school	1	3	10	10	6

Thus sixteen children out of the thirty tested were advanced in the favored school while only five were advanced in the unfavored school. The children from the poorer

[1] *L'Année psychol.*, 1911, 17, 195–196.

[2] Morlé, M., 'L'influence de l'état social sur le degré de l'intelligence des enfants,' *Bull. Soc. libre Educ. Psychol. de l'enfant*, 1911, 12, 8–15.

section were on the average about one fourth year behind the level of their age, while those of the favored school averaged from one fourth to one half year advanced, or a difference of about three fourths of a year between the two social classes.

In 1910 the teachers of the Breslau schools in Germany made a comparative study of children of different social classes there. The demand for a common school for all classes to replace the Vorschule and Volkschule had arisen in Germany. The Volkschule is the elementary public school attended by the children of the laboring and lower business classes, while the Vorschule is attended by the children of the higher social classes. In Prussia the children could enter the Gymnasium, which has a nine-year curriculum preparing for the university after three years of preparation in the Vorschule but only after four years in the Volkschule. The purpose of this investigation was to find whether the mental maturity of the child, as well as the curriculum, justified this. One hundred and fifty-six boys were tested from the two schools. The Binet-Simon scale modified by Bobertag was used. The boys tested were seven and nine years of age from the Vorschule and seven, nine, and ten years from the Volkschule. It was found that the nine-year Volkschule pupils scored 10 percent lower than the pupils of the same age in the better school, while the ten-year-old Volkschule boys attained only the average of the nine-year-old Vorschule pupils. The difference in average was due largely to the fact that the Vorschule pupils did nearly twice as well as the Volkschule pupils of the same age in tests above their age level. The tests at the age level were passed about equally well by both schools. This raises the question of whether children of higher social classes mature earlier than those of the lower levels.[1]

Children from three very different environments were tested in 1911 by J. and R. Weintrob.[2] There were about sev-

[1] Hoffman, A., "Vergleichende Intelligenzprüfungen an Vorschülern und Volkschülern," *Zsch. f. Angew. Psychol.*, 1914, 8, 102–120.

[2] Weintrob, J. and Weintrob, R., "The Influence of Environment on Mental Ability as Shown by the Binet-Simon Tests," *J. of Educ. Psychol.*, 1912, 3, 577–583.

enty children of both sexes in each group tested. Group *A* consisted of children from a school attended by children of the wealthy class, with every opportunity for travel, etc. Group *B* was composed of children whose fathers were wage-earners or small business men. Group *C* was composed of children from a Hebrew Orphan Asylum with no real home environment. The schools were compared as to the number of children, testing above, at, or below the norm for their age, using the Binet-Simon scale. The *A* group was found to rank highest, the *C* group next and the *B* group last. The investigators state: "Judging from the results environment does not seem to affect greatly mental capacity, if at all." Instead of the schools ranking *A* and *B* and *C* as might have been expected, the *C* school or Jewish Orphanage ranked a close second to the wealthy school. However, the question of race enters very largely in this study as the children of the Asylum were all Jewish, while those of Group *A* were predominantly American with a few Germans, Jews and Italians, and Group *B* was largely composed of Germans, Italians and some American children. It is very evident, as the investigators say, that in order to judge fairly differences in environmental influences among groups, the conditions within each group must be uniform, and the same races must be judged.

A study involving social status, incidentally, was made in Columbia, S. C. by Miss Strong.[1] Her primary purpose was to investigate the difference between the white and negro children but in order to make a fair comparison she tested white children in both the city schools and in the mill district. The results show that less than six percent of the city school children were retarded while eighteen percent of the mill district children were mentally over a year below the level of their age. None of the mill district children were above their age level, although ten percent of the city children scored above their years. Approximately the same percent of the children in each district were at the level of their age; eighty-four percent in the city schools and eighty-one per-

[1] Strong, A. C., '350 White and Colored Children Measured by the Binet-Simon Measuring Scale of Intelligence; A Comparative Study,' *Ped. Sem.*, 1913, 20, 485–513.

cent in the mill district schools. Practically the same course of study was used in the schools of both districts.

One of the most recent investigations on the subject of social status was made by Yerkes and Anderson[1] in Cambridge, Mass. In this investigation the Yerkes-Bridges Point Scale[2] was used. Fifty-four children in the kindergarten and first grade of school *A* were compared with children of the same sex and approximately the same age in school *B*. School *A* is located in a good neighborhood and the sociological status of almost all the pupils is very good. School *B* on the contrary is located in a medium to poor section of the city and the majority of its pupils live in a rather poor environment. The children compared were all of English-speaking parents.

The average number of points scored in the two schools is indicated below:

	Age				
	4 Yrs.	5 Yrs.	6 Yrs.	7 Yrs.	8 Yrs.
School *A*	15	27	42	49	56
School *B*	17	22	29	35	41

The favored school averages much higher except in the four-year group. The very young children of the unfavored group seem to have the advantage here, probably because they are less timid. The results show that there is a difference of from twenty percent to thirty percent in mental ability which may be associated with differences in sociological status.

The authors point out that in view of a difference so marked between children of different sociological levels, it is very unfair to judge them by the same norm and that further investigating should be done with the view of establishing norms for different social levels.[3]

[1] Yerkes, Robt. and Anderson, Helen, 'The Importance of Social Status as Indicated by the Results of the Point Scale Method of Measuring Mental Capacity,' *J. of Educ. Psychol.*, **6**, No. 3, Mar., 1915.

[2] Yerkes, Bridges and Hardwick, 'A Point Scale for Measuring Mental Ability,' Warwick and York, 1915. Hereinafter referred to as the "Point Scale."

[3] In a book published since this paper was written, Prof. L. M. Terman discusses the influence of social status. He reports a difference of one to two years between the superior and inferior classes—a result in close conformity with those mentioned above. 'The Measurement of Intelligence,' pp. 72 and 115, Warwick and York, 1916.

Experimental

The Yerkes-Bridges Point Scale was used in this investigation and three hundred and one children were tested in two schools situated in very different localities of Columbus, Ohio. The children from school *A* situated in the better district will be designated as the favored group, while those of school *B* will be designated as the unfavored group.

School *A* is in a very good residence section near the university. The majority of the people own their homes, which are surrounded by well-kept lawns. A portion of the university campus, as well as many wooded lots afford ample playground for the children. The school building is modern in every respect, having been completed about six years ago. The children of the first and second grades spend alternate half hours in a well-equipped Portable where their play is supervised by a teacher who has specialized in this work. This school is considered one of the most desirable in which to teach in the city and only well qualified teachers obtain the positions.

The chief occupations of the fathers of the children in this district were managers, proprietors and officers of manufacturies and stores, traveling salesmen, real estate and insurance agents, and a professional group composed of professors, doctors, lawyers, architects, and ministers. A more complete analysis and grouping of the data by the occupations of the fathers will be given later in this paper. All the children of the first and second grades were tested, and as time did not permit completing the third grade, the children were taken alphabetically. All the children in the grades tested were American born and of English-speaking parents. The testing was done in a hall where occasionally some one passed but otherwise there was no disturbance; and no third party was present when the examination was made. The child's name, date of birth and father's occupation were recorded in every case and checked by the teacher's record.

School *B* is situated near the railroad in a poor factory district of Columbus. The houses average about four or five rooms and are usually built very near the street. They are

often in very bad repair, having been in that part of Columbus which was flooded three years ago. The usual rent is from $8 to $10 a month. Where there are yards they are so ill-kept and muddy that the street is the common playground f r the children. The school house is old and has no inside toilet facilities or up-to-date equipment. There is one saloon on the corner opposite the school building and two others within a block and a half of it. The fathers, if still in the family, are receiving low and irregular wages and almost one half of the fathers of the children in this district belong to the unskilled and casual labor group. The remainder were in the more skilled mechanical trades or were teamsters or delivery men. The mothers are often away all day working in the factories or doing laundry work to supplement the husband's income, or in many cases to support the family entirely. The parents have little idea of the value of education and the children often stop school and go to work as soon as they can secure their working papers. The Associated Charities say that probably 50 percent of the families in this district are registered with some kind of philanthropic organization. The children are often very poorly clad, far from clean and frequently undernourished. Many of the teachers in this school are young and have not taught a great while.

In the case of several children scoring lowest, other members of the family were tested. In five cases the mothers scored only from 47 to 54 points or a mental age of about eight years, and sisters and brothers were far below their age level.

The negroes and children of non-English speaking parents were excluded in this school, but every other child in the first, second and third grade was tested, making a total of 136 children. The testing was done in a small room free from all disturbing elements.

In both schools the tests were given during school hours by one examiner[1] and in all cases doubtful credits were discussed and decided upon by the authors jointly. The child's

[1] Miss Coler.

age to the nearest month was determined by subtracting the date of the birth from the date the tests were given. A given age group includes all children from the middle of the year below to the middle of the one above. Thus, in the group of six-year-olds are included all boys (or girls) from five years seven months to six years six months, inclusive.

Results

The results for the total 301 Columbus school children will first be considered and their scores compared with the scores for Cambridge, Mass., school children of the same ages. These results are presented in Table I. The first

Table I

Age	Columbus			Cambridge			
				English		Non-English	
	No.	A.	M.	No.	A.	No.	A.
6	37	34.1	33	55	29	16	27
7	97	42.8	45	48	35	25	31
8	81	54.7	55	47	41	14	37
9	59	57.3	59	43	56	31	48
10	14	55.9	56	53	62	23	56
11	7	49.6	50	55	65	24	62
12	5	54.8	55	40	77	20	67

A.—Average.
M.—Median.

column gives the ages, the second the number of Columbus children tested at each age, the third and fourth the average and the median scores for Columbus children at each age, while columns five and six give for comparison the number and average scores for Cambridge children of English-speaking parents and columns seven and eight the same for children of non-English-speaking parents.[1]

The results are shown graphically in Fig. 1. Graph *A* is for Columbus children, both schools combined; Graph *B* for Cambridge, English-speaking group, and *B′* for Cambridge, non-English-speaking group. After nine years the number of Columbus children at each age is very small and after ten years composed of children from the unfavored school alone.

[1] Point Scale, pp. 66–67.

This selection is clearly shown by the drop in the curve after nine years, and is due to the fact that in our present school system a child is behind if he is over nine years of age and in the third grade.

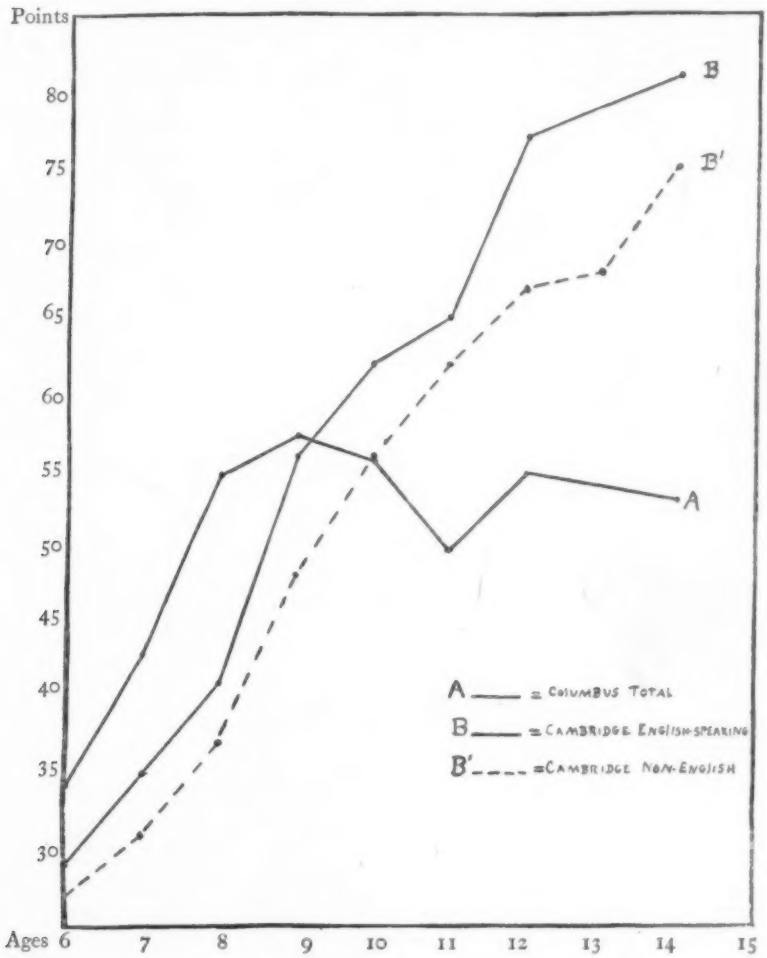

FIG. 1.

The most striking feature in these results is the evident superiority of the Columbus children up to ten years; but it will be shown in the sequel that their superiority is entirely due to the influence of the favored group and that when the

groups are separated the correspondence between the children of the two cities is even closer than might have been expected.

TABLE II

		Age				
		6 Yr.	7 Yr.	8 Yr.	9 Yr.	10 Yr.
Cambridge Heterogeneous	Number	71	73	61	74	76
	Score	29	34	39	52	59
Total Columbus	Number	37	97	81	59	14
	Score	34.1	42.8	54.7	57.3	55.9
Combined Average	Number	108	170	142	133	90
	Score	30.7	39.0	48	54.4	58.5

The Columbus results from the sixth to the tenth year inclusive may be combined with the Cambridge results for the same ages to help in the standardization of the scale.

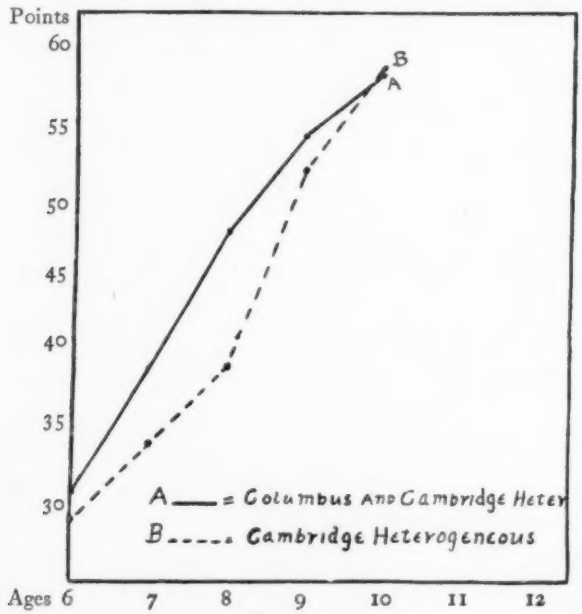

FIG. 2.

At first the Columbus results were combined with the Cambridge heterogeneous group[1] in the following way. The number of children at each age in the group was multiplied by the average score of that age for Columbus and Cambridge

[1] Point Scale, pp. 64-65.

separately. Then these results were added and divided by the total number at that age. Table II. shows the average score and number of pupils at each age in each group and the combined averages or new norms obtained. The latter are also shown graphically in Fig. 2. The solid line represents the combined norm for the total 643 children of Columbus and

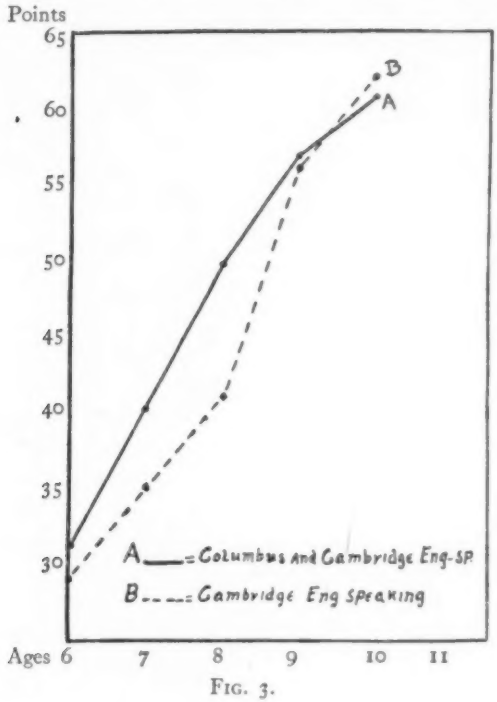

FIG. 3.

Cambridge; and the dotted line is the original norm from six to ten years for the 355 Cambridge children alone.

As all the children tested in the Columbus schools were of English-speaking parents it seemed fairer to use as a standard for judging the Columbus pupils a norm made from the Columbus results combined in the same way with the Cambridge group of children of English-speaking parents,[1] instead of the heterogeneous group.

These norms are shown in Table III. and graphically in Fig. 3. In this case the solid line represents the combined

[1] Point Scale, pp. 66-67.

norm for 245 Cambridge children of English-speaking parents and 288 Columbus children, or a total of 534 children. The broken line shows the original norm for the Cambridge English-speaking group alone. Selection entering after nine years in the Columbus results will explain the slight drop in the solid curve from nine to ten years. This curve was used as a norm for determining the mental age and coefficient of

TABLE III

		Ages				
		6 Yr.	7 Yr.	8 Yr.	9 Yr.	10 Yr.
Cambridge English-speaking.....	Number....	55	48	47	43	53
	Score	29	35	41	56	62
Total Columbus.....	Number....	37	97	81	59	14
	Score	34.1	42.8	54.7	57.3	55.9
Combined Average.....	Number....	92	145	128	102	67
	Score	31.1	40.2	49.7	56.8	60.7

mental ability of the Columbus children. For scores above or below the six, seven, eight, nine and ten year averages the norm for the English-speaking group of Cambridge alone had to be used.

COMPARISON OF DIFFERENT SOCIAL GROUPS

The Columbus results from Schools *A* and *B* will now be compared. Tables IV. and V. show the individual scores by sex and age for the favored and unfavored schools respectively. The average score for each age and each sex and also for both sexes combined are likewise given in each table.

These results are shown graphically by Fig. 4, curves *A* and *B*. The favored school is from 21 percent to 32 percent superior to the unfavored school varying with the chronological age. The curve drops at nine years in School *A* and at ten years in School *B*, showing that the selection previously mentioned enters earlier in the former. After ten years the numbers in the unfavored group are very small and the scores are not representative of these age groups.

As these results are very similar to those found in the Cambridge investigation it is interesting to compare them

TABLE IV

FAVORED *A*

6		7		8		9		10	
(8) M.	(9) F.	(36) M.	(23) F.	(26) M.	(27) F.	(16) M.	(14) F.	(2) M.	(4) F.
——	....		——	....	——	——	....	——	
29	31	22	32	49	36	44	54	66	39
30	36	——	....	51	41	....	55	68	——
....	——	34	36	51	....	50	61	——	49
36	39	35	38	52	45	52	62		64
——	39	35	40	52	49	56	63		75
40	41	....	43	52	49	60	63		——
42	42	40	45	53	51	61	65		
47	43	41	45	53	51	62	65		
47	....	42	49	55	55	63	67		
48	52	43	49	55	55	64	68		
....	54	45	49	55	55	64	68		
		47	49	56	56	66	71		
		47	——	58	57	69	——		
		47	50	58	58	69	72		
		48	51	——	59	——	73		
		48	51	60	59	73	....		
		48	51	62	——	77			
		49	52	63	60	79			
		——	52	64	60	....			
		50	52	65	62				
		50	53	66	63				
		52	53	66	63				
		54	55	67	65				
		54	59	69	66				
		54	....	72	67				
		54	67	....	67				
		55		75	69				
		55		76	71				
		56			73				
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		72							
Ave.....39.8	41.9	48.1	48.7	59.8	57.9	63.1	64.8	67	56.8
40.9		48.4		58.8		64		60.2	

—— = 25 percent above or below general norm.
.... = 25 percent above or below group norm.

TABLE V

UNFAVORED *B*

6 Yr.		7 Yr.		8 Yr.		9 Yr.		10 Yr.		11 Yr.		12 Yr.		14 Yr.
(14) M.	(6) F.	(24) M.	(14) F.	(14) M.	(14) F.	(11) M.	(18) F.	(4) M.	(4) F.	(6) M.	(1) F	(3) M.	(2) F.	(1) M.
					....			....	....					
16	17	14	19	22	35	35	24	43	45	36	46	46	55	53
21	....	21	22	25		35	....			48		53		
....	23	25	22	27	38	....	42	54	50			57	60	
22		25	24	....	42	38		57	52	50				
23	28	26	....	36	44		44	60	61	55				
23	29	26	28		45	43	44	....	....	58				
	29	....		39	48	47	46			72				
25		27	34	44	49	49	47							
25	....	27	37	45	49	52	50							
25	42	28	37	45	51	53	50							
26		29	43	46	52	58	51							
29		29	....	48	57	58	53							
33		29	44	50	57	59	54							
34			45	52	57		54							
....		32	45			....	58							
		32		....	....		59							
39		35	54	62	68		61							
55		37	58	62			61							
		40					....							
		41					67							
		41												
		42					75							
		43												
		....												
		44												
		50												
		54												
Ave.....28.3	27.2	33.2	36.6	43.1	49.4	47.9	52.2	53.5	52	53.2	46	52	57.5	53
28.3		34.4		46.2		50.1		52.8		52.1		54.2		53

—— = 25 percent above or below general norm.

.... = 25 percent above or below group norm.

with the two similar groups there.[1] Table VI. shows these results and they are also shown graphically in Fig. 4. Only the kindergarten and the First Grade were tested in the Cambridge favored school so the curve is short.

TABLE VI

		Favored School					Unfavored School				
	Age.....	6 Yr.	7 Yr.	8 Yr.	9 Yr.	10 Yr.	6 Yr.	7 Yr.	8 Yr.	9 Yr.	10 Yr.
Cambridge..	Score..	42	49	56	.....	.....	29	35	41	56	62
Columbus...	Score..	40.9	48.4	58.8	64	60.2	28.2	34.4	46.2	50.1	52

[1] Point Scale, p. 74, p. 66.

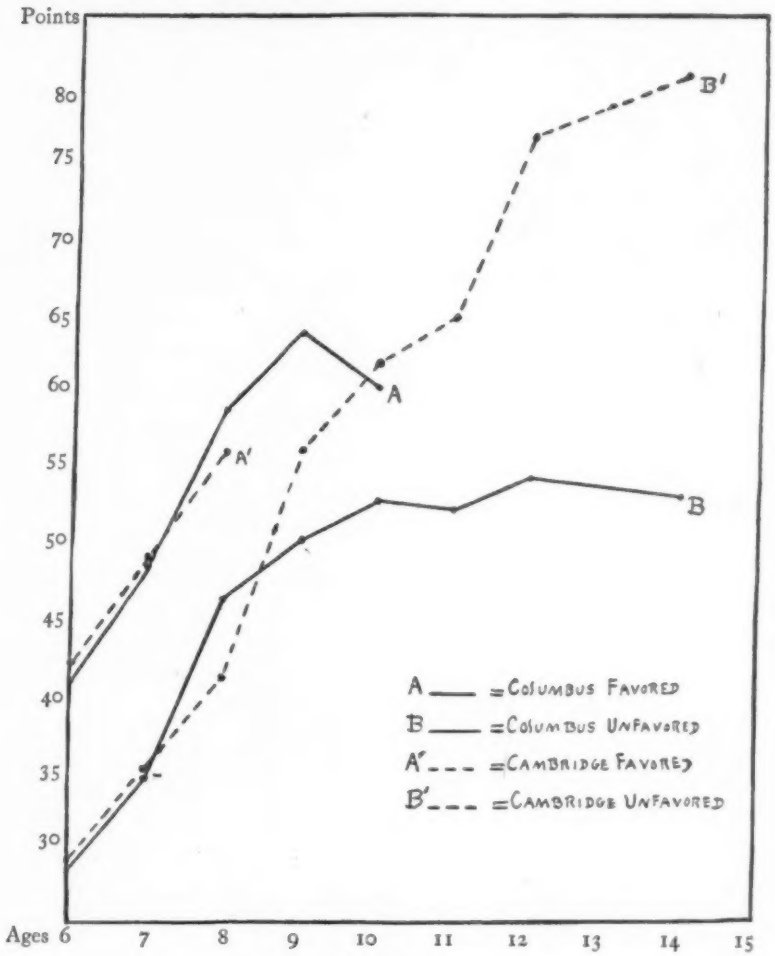


FIG. 4.

TABLE VII

		School *A*		School *B*	
		M.	F.	M.	F.
Cambridge.....	No.....	11	13	29	26
	Score.....	42	40	29	30
Columbus.....	No.....	8	9	14	6
	Score.....	39.8	41.9	28.3	27.2

These differences between the favored and unfavored groups and the striking similarity between the Columbus

and Cambridge results are even more clearly shown by the data for the six-year-old groups given in Table VII.

Comparison of Sexes

Tables IV. and V. should be consulted for the individual scores. The averages for boys and for girls in both schools combined are shown in Table VIII.

Table VIII

	6 Yr.	7 Yr.	8 Yr.	9 Yr.	10 Yr.
Males	32.5	42.2	54	56.9	58
Females	36.3	44.1	55	57.7	54.4

Fig. 5 shows these results graphically. The superiority of the girls over the boys seems marked up to ten years. By examining Table IX. and Fig. 6, which give the averages for

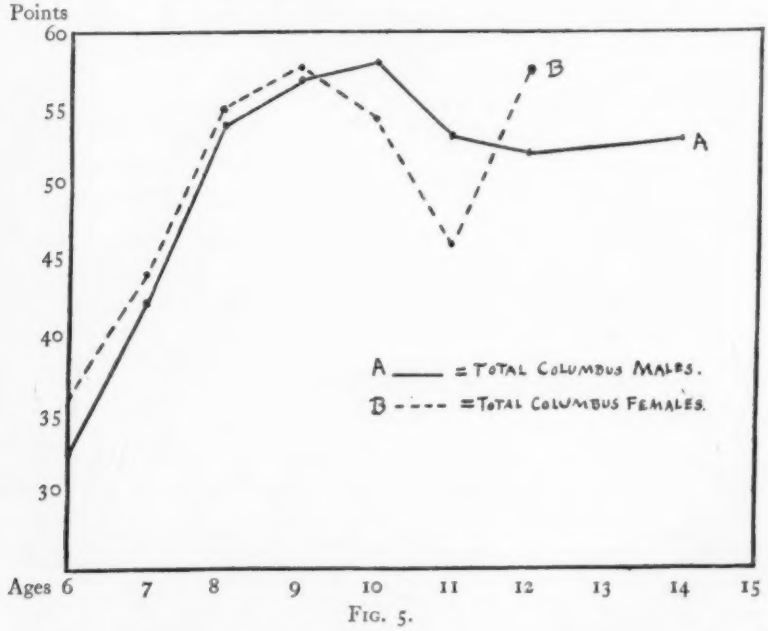


Fig. 5.

each sex in each school separately it will be seen that this superiority of the girls is brought about almost entirely by the girls of the unfavored school.

TABLE IX

	6 Yr.		7 Yr.		8 Yr.		9 Yr.		10 Yr.	
	M.	F.	M.	F.	M.	F.	M.	F.	M.	F.
Unfavored	28.3	27.2	33.2	36.6	43.1	49.4	47.9	52.2	53.5	52
Favored	39.8	41.9	48.1	48.7	59.8	57.9	63.1	64.8	67	56.8

The difference between the sexes is particularly marked in the eight-year group of the unfavored school, where there is an equal number of boys and girls. At the same age in the favored school the boys have the advantage. These

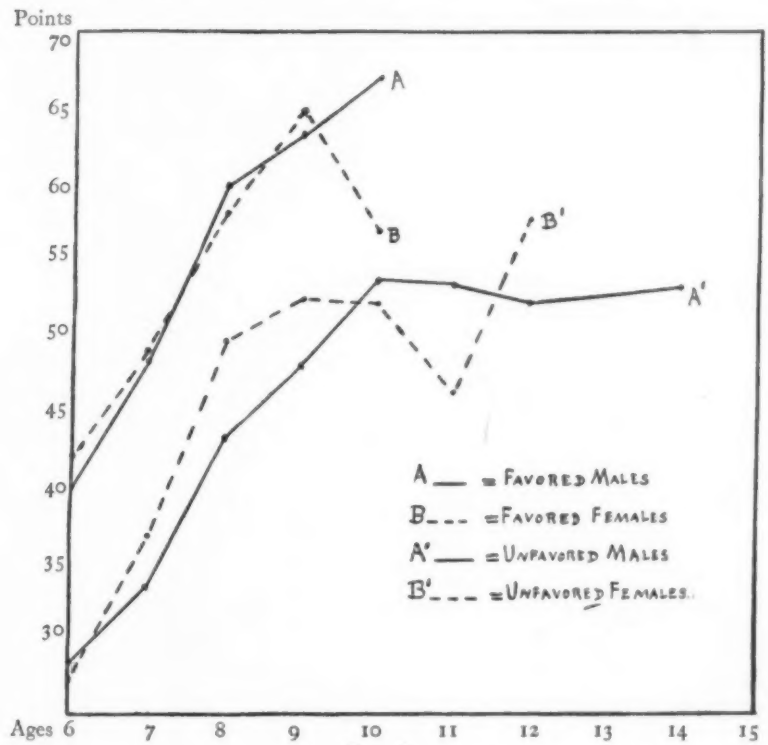


FIG. 6.

results seem to bear out the theory held by Thorndike[1] and others that females deviate less from the norm than males; for there is less difference between the performances of the girls in the two schools than there is between the performances of the boys.

[1] 'Educational Psychology', second edition, 1910, pp. 33–43.

TABLE X

	First Grade				Second Grade				Third Grade			
	No.	M.	A.	Age	No.	M.	A.	Age	No.	M.	A.	Age
Favored	57	47	45.3	6–7½ mo.	55	55	56.2	7–8 mo.	53	64	63.5	8–8½ mo.
Unfavored ..	52	27.5	29.1	6–9½ mo.	44	45.5	46.2	8–5 mo.	40	54.5	55.1	9–9 mo.
Difference ...	...	19.5	15.7	2 mo.	...	9.5	10	4½ mo.	...	9.5	8.4	1 yr.

M.—Median.
A.—Average.
Age—Chronological age in years and months.

TABLE Xa

First Grade				Second Grade				Third Grade			
Favored		Unfavored		Favored		Unfavored		Favored		Unfavored	
M.	F.	M.	F.	M.	F.	M.	F.	M.	F.	M.	F.
57		52		55		44		53		40	
22	31	14	17	44	36	29	24	50	49	43	44
29	32	16	19	49	39	35	35	52	49	45	45
30	36	21	22	50	41	36	37	52	51	48	46
34	36	21	22	51	45	37	42	53	54	50	46
35	38	22	23	51	49	39	42	53	55	53	50
35	39	22	24	52	49	40	43	56	56	53	50
36	39	23	28	52	49	42	44	58	57	54	50
40	40	23	28	54	51	43	44	60	58	55	51
40	41	25	29	54	51	43	44	60	61	57	51
41	42	25	29	55	51	44	45	61	63	57	52
42	43	25	34	55	52	44	47	62	63	58	53
42	43	25	37	55	53	45	49	62	64	58	54
43	45	25	38	55	55	46	49	63	65	58	54
45	45	25	42	56	55	46	52	64	65	59	55
47	49	26	45	56	55	47	54	64	65	60	57
47	49	26	45	56	55	48	57	64	67	62	58
47	50	26	48	58	59	49	57	66	67	72	59
47	51	27		58	59	50	58	66	68		60
47	52	27		58	60	50	67	67	68		61
48	52	27		59	60	52		68	71		61
48	52	28		63	62	52		69	71		61
48	53	29		64	62	53		69	72		68
48	54	29		65	63	54		69	73		75
49	59	29		66	66	55		72	73		
50	63	32		66	67	62		73	75		
52		32		66	67			76			
54		33		72	69			77			
54		34		75				79			
55		35									
60		35									
64		36									
71		38									
		39									
		41									
		41									
Ave.....45.3	45.4	28.1	31.2	57.7	54.8	45.6	46.8	63.8	63.2	55.4	54.8
45.3		29.1		56.3		46.2		63.5		55	

Comparison of Grades

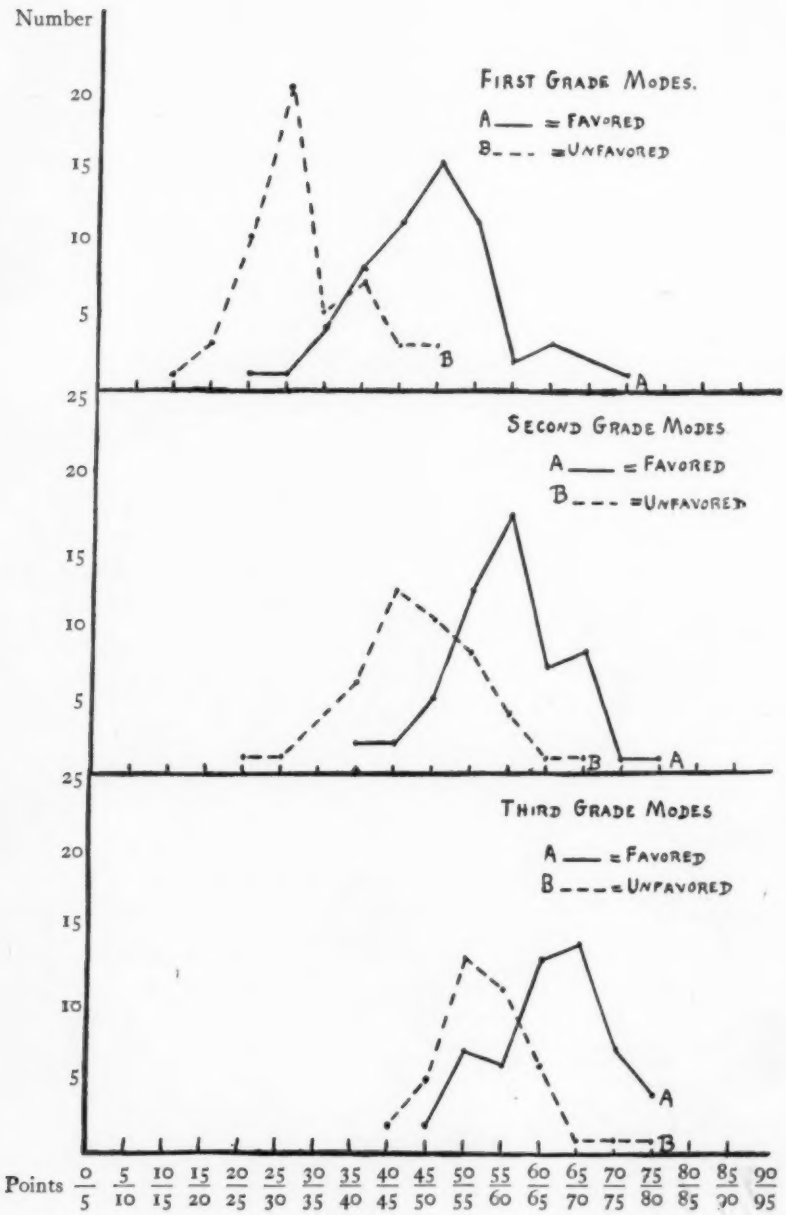
Table X. gives in convenient form the number of pupils, the median and average scores and the average chronological ages for each grade in each school. The individual scores for each school and each grade are given on Table X*a*.

It will be seen that the superiority of the favored school is much greater for the first than for the second or third grades.

By examining the Figs. 7, 8, and 9, showing the modes for the three grades, the same thing will be found to be true. The favored school, first grade, has a mode of from 45 to 50 points and the unfavored school from 25 to 30 points or a difference of 20 points. The difference in the second grade is 15 points and in the third the difference lies between 10 and 15 points as the mode is not distinct here.

These results seem to show that the difference between the children of the two schools is greatest when they enter the school and that it becomes less from grade to grade. This would mean that the school work tended to equalize some of the original difference.

It is necessary, however, to examine further to see if there may be another explanation before accepting this conclusion. The average chronological ages for the three grades are also given in Table X. The ages are nearest the same in the first grade where there is only two months difference, but this difference increases to over a year in the third grade. This age difference is brought about by the fact that in the unfavored school so many children are in a low grade for their years. In our present school system a child enters at the age of six and would therefore be in the third grade at the age of eight to nine years if regularly promoted. Out of the forty children in the third grade of the unfavored school there were eight in the ten-year group, five in the eleven-year group, four in the twelve-year group and one in the fourteen-year group. That is, about 45 percent of the children in that grade are older than they should normally be in the third grade. On the other hand there are only five older than the nine-year group in the third grade of the favored school and none of these is above ten years six months.


Number
FIRST GRADE MODES.
A ___ = FAVORED
B - - - = UNFAVORED
SECOND GRADE MODES
A ___ = FAVORED
B - - - = UNFAVORED
THIRD GRADE MODES
A ___ = FAVORED
B - - - = UNFAVORED
Points 0/5 5/10 10/15 15/20 20/25 25/30 30/35 35/40 40/45 45/50 50/55 55/60 60/65 65/70 70/75 75/80 80/85 85/90 90/95

FIGS. 7, 8, AND 9.

If we now compare the average scores for each age group in the two schools we find that the differences from year to year are fairly constant. These figures are shown in Table XI.

TABLE XI

	6 Yr.	7 Yr.	8 Yr.	9 Yr.	10 Yr.
Favored	40.9	48.4	58.8	64	60.2
Unfavored	28.3	34.4	46.2	50.1	52.8
Difference	12.6	14	12.6	13.9	7.4

The difference is practically the same for the six- and seven-year groups and the decrease at ten years is probably due to selection entering in both schools at this age. Moreover there are too few ten-year-old children for a fair comparison. There is thus very little diminution of the initial difference with age; and the fact that the difference in mental ability between the two schools is less in the second and third grades than in the first, must therefore be largely due to the older children in the second and third grades of the unfavored school who tend to raise the average of points scored in that grade and so make it approach nearer to the favored school's results.

A coefficient of mental ability for each child was obtained by dividing the number of points actually scored by the number of points which should be scored at his chronological age. The standard used was the one obtained by combining the total Columbus results with the results of the Cambridge children of English-speaking parents.[1] The mental age of the child can also be obtained by referring his score to the age at which this score should be obtained.

The average coefficient of mental ability, average mental age and average chronological age for each grade in each school are shown in Table XII.

There is a uniform difference of about a year in the average chronological age of the children from grade to grade in the favored school. On the other hand there is a difference of almost two years between the average chronological

[1] See Table III.

age of the children in the first grade and those of the second grade in the unfavored school. This indicates that the children of the first grade in the unfavored school remain on the average two years in that grade before being promoted. Moreover the mental age for the children of the unfavored school is about a year lower than their actual chronological age. It would therefore seem that the children of the unfavored group are not mature enough mentally when they enter school at the age of six to be able to do the prescribed

TABLE XII

	Favored			Unfavored			Difference		
Grade	I	II	III	I	II	III	I	II	III
C. A.....	6–7½ mo.	7–8 mo.	8–8½ mo.	6–9½ mo.	8–5 mo.	9–9 mo.	2 mo.	4½ mo.	1 yr. 5 mo.
M. A.....	7–9	9–3	10–4	5–9	7–9	8–11	2 yr.	1 yr. 6 mo.	1 yr. 5 mo.
C. M. A...	1.24	1.26	1.18	.77	.98	.93	.47	.28	.25

C. A., chronological age.
M. A., mental age.
C. M. A., coefficient of mental ability.

work. The children of the favored group, on the contrary, are mentally a year ahead of their chronological age and so these children might equally well enter school at an earlier age and be able to do the required work. It is also interesting to note in this table that although the difference in the mental age between the two schools decreases from grade to grade, the difference in their chronological age increases, thus keeping a fairly constant difference of two years between the schools in the grades tested. Binet estimated that there was a difference of about one and one half years between children of different social classes, and M. Morlé found about a year's difference in the groups he studied in Paris.

COMPARISON OF SEPARATE TESTS

Table XIII. shows the average scores for each of the twenty tests making up the Point Scale. Scores for the favored and unfavored boys and girls are given separately and the combined average and the difference between the schools is also given. The difference, although sometimes very small, is always on the side of the favored school.

TABLE XIII

Test	Fav. Boys	Unfav. Boys	Fav. Girls	Unfav. Girls	Total		Diff.
					Fav.	Unfav.	
1	2.90	2.62	2.98	2.91	2.94	2.77	.17
2	3.73	3.18	3.88	3.22	3.8	3.2	.60
3	2.97	2.77	2.98	2.71	2.98	2.74	.34
4	3.18	2.48	3.19	2.67	3.19	2.55	.64
5	3.27	2.02	3.24	2.67	3.26	2.35	.91
6	2.22	1.87	2.20	1.94	2.21	1.91	.30
7	6.76	5.90	6.76	6.	6.76	5.95	.81
8	1.73	1.46	1.54	1.57	1.64	1.52	.12
9	4.58	3.06	4.28	3.59	4.43	3.33	1.10
10	5.42	3.98	5.18	4.64	5.30	4.31	.99
11	2.09	1.48	2.25	1.32	2.17	1.40	.77
12	2.43	2.06	2.27	2.25	2.35	2.16	.19
13	1.51	.78	1.64	1.40	1.58	1.09	.49
14	1.66	.52	1.16	.98	1.41	.75	.66
15	3.40	2.27	3.50	2.40	3.45	2.34	1.11
16	1.65	1.09	1.38	1.45	1.52	1.27	.25
17	2.05	.79	2.01	.86	2.03	.83	1.20
18	1.38	.39	1.37	.88	1.38	.64	.74
19	.89	.42	1.19	1.05	1.04	.74	.30
20	1.30	.75	1.35	.90	1.33	.83	.50

The five tests in which the greatest superiority of the favored school is shown are given here in the order of the amount of difference.

Test number 17, absurd statements, is described by the authors[1] of the Point Scale as primarily a test for 'logical judgment based on imagination, analysis and reasoning.'

Number 15, comprehension of questions, tests 'practical judgment involving memory and imagination.'

Number 19, comparison of familiar objects involves 'analysis and comparison of remembered objects and attention.'

Number 10, concrete definitions, tests 'ideation (association) and analysis.'

Number 5, which shows the fifth greatest difference between the two schools, consists of counting backward from 20 to 1. Here the mental traits involved are 'memory, imagination and attention.' Probably the reason for the higher average score in the favored school is the fact that games involving counting backwards were played by the children of this school and when they were given this test

[1] Point Scale, p. 8.

they knew what was expected, with little explanation; while counting backward seemed a new process for most of the younger children of the unfavored school.

The results of test number 19, abstract definitions, would probably have shown a greater difference between the two schools but for the fact that the average for the unfavored school was raised because a greater number of unfavored children were able to define 'charity.' The familiarity with this term is easily understood, as charity in some form is extremely common in the unfavored district.

With the exception of number 5 it seems, then, that the greatest difference in the performances of the children of the two schools lies in the tests involving primarily analysis and abstraction. This agrees with Binet's analysis of the difference between the scores of the children in the private school at Brussels and those from the poorer section of Paris. Binet found the Belgian children superior in tests involving criticism, comparison, abstract definitions, absurdities, or in those involving thought in the higher sense. He pointed out that some of these tests probably put a premium on the ready use of language and that the children from the higher social class had the advantage in this respect.

The five tests which show the least difference between the two schools will now be considered. Number 8, arranging weights, is described as having to do with 'kinæsthetic judgment, ideation and attention.'

In number 1, æsthetic judgment, the difference between the two schools is also very slight. This is described as having to do with 'æsthetic judgment involving perception, association and analysis.' As this is probably the easiest of all the tests, it was seldom missed by either school, which probably accounts for the small difference in the average scores.

'Motor coördination and visual perception' are the traits involved in Number 12, copying a square and a diamond.

Number 3, comparison of lines and weights, tests 'discrimination' of the visual type in the first part and of the kinæsthetic type in the second.

Number 16, drawing of designs from memory, involves 'visual memory, perception, attention and motor coördination.'

With the exception of number 1 the above tests all involve sensory-motor functions to a great extent and have to do primarily with kinæsthetic judgment and motor coördination.

To summarize: The results from the single tests show the greatest difference in tests involving analysis and abstraction and the least difference in those involving primarily motor coördination and kinæsthetic judgment. This agrees with Thorndike's view that individuals differ least in sensory motor functions and most in analysis and abstraction.[1]

Individual Scores

The individual scores will now be considered as to the number of children who are twenty-five percent above or below the norm of their age, when judged by the general norm and when judged by the average of their own school. Tables IV. and V. show the individual scores by sex and age groups for each school. In each group the number of individuals whose scores depart by twenty-five percent or more

Table XIV

		No.	Percent of Total
Using general norm.....	Favored group, 25% below the norm...	3	1.8
	Favored group, 25% above the norm...	73	44.2
	Unfavored group, 25% below the norm.	44	32.4
	Unfavored group, 25% above the norm.	11	8.1
Using separate norms for each school.....	Favored group, 25% below the norm...	10	6.2
	Favored group, 25% above the norm...	10	6.2
	Unfavored group, 25% below the norm.	19	16.5
	Unfavored group, 25% above the norm.	16	13.9

from the norm for that age is indicated. The solid lines indicate the individuals who deviate twenty-five percent or more from the general norm for that age group. The dotted lines indicate the number who deviate twenty-five percent or more from their own group norm. The exact numbers are shown in convenient form in Table XIV.

[1] Thorndike, E. L., 'Educational Psychology,' second edition, 1910, pp. 218–223.

As will be seen, nearly one third of the children of the unfavored school have a coefficient of mental ability of .75 when judged by the general norm. This has been suggested by Dr. T. H. Haines[1] as a criterion of feeble-mindedness. He has shown that it is a more lenient criterion than four years' retardation above the thirteenth year. For the ages here considered it is roughly equal to about two years' retardation. The number of the favored group twenty-five percent or more below the general norm for their age is only 3. On the other hand the favored school shows over 44 percent, twenty-five percent or more above the norm, while the unfavored school has only eight percent. If these results are compared with those of Cambridge[2] where practically the same numbers are twenty-five percent above and below the average, it appears that the unfavored school is greatly weighted by subnormals and the favored school by supernormals. The question now arises whether it is fair to judge both schools by the same standard. Let us see what the results would show if the unfavored and the favored schools were judged by their own norm or average. In this case the standard will of course be lowered for the unfavored group and raised for the favored group. The dotted lines indicate this in Tables IV. and V. The averages of the pupils were only used up to and including nine years, however, for beyond nine years the scores were so low and there were so few cases that a fair average could not be obtained. Up to the ten-year group, then, the number twenty-five percent above and below the norm for each school is about equal, as was the case in the Cambridge schools.

Considering the great number which must be classed as very inferior intellectually, if not feeble-minded, in the unfavored school if the same standard is used for judging both schools, it seems unfair that groups of children from such different social classes should be judged by the same norm. If the sociological factor is not considered in clinical diagnosis, it seems probable that too high a standard will be expected

[1] Haines, T. H., 'Relative Values of Point Scale and Year Scale Measurements of 1,000 Minor Delinquents,' *J. Exp. Psychol.*, 1, 51–82.

[2] Point Scale, p. 55.

of the unfavored individuals and so the degree of mental deficiency which might exist would be overestimated.

Study of Occupations

The children will now be grouped in the two schools by the occupation of the father. The 165 children in the favored school were classified as follows:

(1) Professional group	32
Professors	17
Doctors	6
Lawyers	3
Editors	3
Architects	2
Ministers	1
(2) Proprietors, officers and managers of manufacturies and stores	32
Proprietors	16
Managers and officers	11
Building contractors	5
(3) Traveling salesmen, insurance agents and real estate dealers	39
Traveling salesmen	33
Salesmen	5
Insurance agents	6
Real estate dealers	6
(4) Clerical workers	21
Clerks	13
Bookkeepers and accountants	5
Cashiers	3

(5) The remaining forty-one children were classified in a miscellaneous group.

The main groups in which the 136 children of the unfavored school were classified are as follows:

(1) Laborers, unskilled	60
This group includes odd-job workers and all unskilled and casual laborers.	
(2) Skilled mechanical trades	45
Railroad engineers and mechanics	13
Metal workers	12
Building trades	10
Electricians	3
Shoe cutters	3
Miscellaneous skilled workers	4
(3) Teamsters and delivery men	19

The remaining twelve children in the unfavored school were put in a miscellaneous class.

In comparing the groups in the favored school, the children from one group were matched with children as nearly as

possible the same age in the other group. The results are here shown for the professional group compared with the traveling salesmen. Thirty children from each group were matched in this comparison.

	Av. Chron. Age	Av. Mental Age	C. M. A.
Professional group	7 yr. 7 mo.	9 yr. 8 mo.	1.42
Traveling salesmen	7 yr. 7 mo.	9 yr. 3 mo.	1.26

The average chronological age is thus the same, but the professional group averages 5 months superior to the traveling salesmen group mentally.

The clerical workers were rather a mixed group. Many of the clerks were chief clerks and the group as a whole is small. Comparing seventeen from this group with seventeen from the manager group of corresponding ages, the following results were obtained.

	Av. Chron. Age	Av. Mental Age	C. M. A.
Clerical	7 yr. 10 mo.	9 yr. 1 mo.	1.22
Managing class	7 yr. 8 mo.	9 yr. 5 mo.	1.24

The managing class averages two months younger chronologically, but shows about four months' superiority mentally.

In the unfavored school thirty-six children were matched from the skilled and unskilled laboring classes.

	Av. Chron. Age	Av. Mental Age	C. M. A.
Skilled	7 yr. 11 mo.	7 yr. 6 mo.	.93
Unskilled	8 yr. 1 mo.	6 yr. 11 mo.	.80

The unskilled group here has the advantage of being two months older; nevertheless its average mental age is seven months less and its coefficient of mental ability .13 less than the average for the skilled group.

When a group was selected from laborers to match the ages of the eighteen children in the teamster group, the following results were obtained.

	Av. Chron. Age	Av. Mental Age	C. M. A.
Teamsters	7 yr. 10 mo.	7 yr.	.83
Unskilled laborers	7 yr. 10 mo.	7 yr. 2 mo.	.88

The teamsters appear to have as a class an even lower mentality than the unskilled laborers, but this is a very small group and results might be different if larger numbers were compared.

The following is a summary of the results for the various occupation groups, irrespective of schools and ages:

	No.	Av. Chron. Age	Av. Mental Age	C. M. A.
Professional	32	7 yr. 3 mo.	9 yr. 8 mo.	1.42
Traveling salesmen	39	7 yr. 6 mo.	9 yr. 2 mo.	1.26
Proprietors, etc.	34	7 yr. 10 mo.	9 yr. 1 mo.	1.21
Skilled	63	8 yr.	7 yr. 10 mo.	1.12
Unskilled	60	8 yr.	7 yr. 1 mo.	.83

It is noteworthy in this table that although the chronological age increases from group to group, the mental age decreases.

Conclusions

Our results corroborate the conclusions of Binet in France, Hoffman in Germany and Yerkes *et al.* in United States that there is a very considerable dependence of intelligence upon sociological condition. We have further shown that when children are classified according to the occupations of their fathers, a striking correlation is shown between intelligence quotient and occupation group. Hence, if mental age rather than chronological age were used to determine the time for beginning school, the children of the professional group, for example, would begin school two years earlier than the children of the unskilled labor group; for the former mature intellectually much earlier than the latter.

Incidentally the results have shown that the correlation of intelligence and social status is probably higher for boys than for girls. The girls of the poorer school are considerably superior to the boys; but the boys of the better school are only at one age noticeably superior to the girls. ?

must be a mistake somewhere

The superiority of the better classes is most evident in tests that involve higher mental processes like analysis and abstraction; but it is also shown to a lesser extent in sensory motor functions.

We have not discussed the causes of this relation of intelligence to social status for the very good reason that our data do not contribute anything towards a solution of the problem. They aim merely to establish the fact and amount of the difference, and could be used by adherents of the "Environment Theory" as well as by advocates of "Inheritance." Thus, the former could emphasize the quite evident differences in home and school environments, teaching staff, etc.; while the latter would point to the just as evident differences in the character and intelligence of the parents. It is worth noting that in the few cases where the mothers were tested, they showed a mental age about equivalent to that of their children. If intelligence quotient could be obtained for a number of successive generations with different environments, such data might contribute to a solution of the problem.

We have also omitted discussion of the percentage of feeble-minded in the different social groups, and have concerned ourselves only with the variations in intelligence; for we consider the diagnosis of feeble-mindedness and the measurement of intelligence two quite distinct though related problems that had better not be confused. Diagnosis depends upon a number of other considerations as well as the psychological. The physical aspect can not be wholly neglected; and the importance of the patient's life history is generally acknowledged, especially if amentia is to be distinguished from dementia.

There are also sociological considerations which the advocates of a purely psychological concept of feeble-mindedness must acknowledge as soon as they consider the problem of the dividing line between normal and feeble-minded intelligence. The various criteria: two to four years' retardation, an intelligence quotient below .75, the 'lowest three percent'

of the population,[1] etc., are all ultimately based upon sociological, or socio-legal considerations. They are merely statements of the limits below which an individual fails to attain certain social standards of living.

Now, since these standards of living vary greatly from group to group, it seems only reasonable that the above mentioned limits (and intelligence norms) used in diagnosis should vary too. Otherwise, we might be obliged to classify whole races as feeble-minded. All Hottentots would probably be feeble-minded, if judged by Anglo-Saxon intelligence norms; and similarly the majority of the children of the unskilled labor group might be classed feeble-minded if judged by norms for the professional group. The facts discussed in this paper should therefore find a place among the various considerations upon which careful diagnosis depends; but we have preferred to confine ourselves to the strictly psychological problem: the measurement of the intelligence of different social groups.

[1] Pintner, R. and Paterson, D. G., 'A Psychological Basis for the Diagnosis of Feeble-mindedness,' *J. of Crim. Law and Crim.*, 7, May, 1916.

MENTAL TESTS WITH DELINQUENTS AND AUSTRALIAN ABORIGINAL CHILDREN

BY S. D. PORTEUS

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During 1915 two groups of delinquent boys were examined by a new series of mental tests.[1] The first group consisted of boys ranging in age from nine to fourteen years who had been committed to the care of the Boys' Home at Burwood, Victoria. Some few were merely neglected children but the majority had been before the children's courts for various minor offences, chiefly truancy and petty thieving. Under ordinary environment these may be considered criminals in the making.

The tests given are based on the maze plan and the subject is required to find a way through the maze in a given number of trials under certain conditions.[2] Success requires the exercise of prudence in action, forethought, and general mental alertness. The tests are graded for the mental ages from three to thirteen years. It is not claimed that they enable us to arrive at the general mental age of the subject, though in the majority of cases there is a close correlation between results by these tests and by the Binet-Simon. In another investigation, out of one thousand normal children examined 70.6 percent passed by these tests within one year of their Binet ages.

Mental age per the Porteus tests means that, in the capacities of foresight, prudence, resistance to suggestion, and sustaining the attention, the child has reached the average development of the age assigned to the tests passed under the given conditions. Children below ten years of age are most often placed about one year higher in mental age by the Porteus tests than by the Binet. This difference is accounted for

[1] Tests published by C. H. Stoelting Co., Chicago.

[2] See *J. of Exp. Ped.*, June, 1915, or *Amer. J. of Psycho-Asthenics*, June, 1915.

mainly by the fact that the former are motor tests and therefore make a more universal appeal to child interest and secondly because they were arranged so as to permit of a comparison between the mentally deficient and the dull normal child. This explanation is necessary to meet the possible objection that the tests were too difficult.

Since impulsive and ill-considered action had been characteristic of the delinquent boys' social behavior it was thought likely that their performances in the tests would reflect to some degree the same faults of disposition. How this expectation was fulfilled may be seen by reference to Table I. below. In some cases where foresight was shown in the subject's preliminary study of the problem before beginning its working, failure resulted through a too imprudent trust in the memory. Not a few looked at the maze before beginning the test and remarked "I see the way out," and then went impulsively to work only to find that they had lost the plan and had taken a wrong turning, which, of course, meant instant failure, since corrections are not allowed. It was very rare indeed to find a child with the most intelligent method of attack, viz., a preliminary sizing up of the problem and then a careful and deliberative working—prudence and forethought in combination.

The following is a summary of the scores of the boys in the tests as compared with their chronological ages.

TABLE I

Passed Test	
Above age	0
At age	5
1 year below chron. age	3
2 years " " "	5
3 years " " "	5
4 years " " "	3
5 years " " "	1
Total	22
Average chronological age	12 years 8 months
Average test passed	10 " 4 "
Average deficiency	2 " 4 "

It is significant that no boy passed a test above his chronological age while less than 25 per cent. passed 'at age.'

In Table II. some interesting individual records are given together with a brief report by the superintendent of the Home as to each boy's character.

Those in whose personal reports there is a favorable entry are placed in Section A. Those whose social dispositions are not satisfactory are shown in Section B.

TABLE II

SECTION A

Case	Chron. Age	Test Passed	Deficiency	Superintendent's Report
C. D.	11 yrs.	10 yrs.	1 yr.	Quick witted, fairly reliable, and moderately intelligent.
G. T.	12½ yrs.	12½ yrs.	—	Truant, good open disposition fairly intelligent, conduct good.
A. A.	11 yrs. 9 mos.	12 yrs.	—	Fairly intelligent, good disposition.
E. S.	11 yrs.	11½ yrs.	—	Rather dull, but improving wonderfully.
W. G.	13 yrs.	12½ yrs.	½ yr.	Splendid memory, intelligent for his age.
C. C.	12 yrs.	11½ yrs.	½ yr.	Truant. Conduct good, fairly intelligent.
N. S.	12 yrs. 10 mos.	11 yrs.	1½ yrs.	Truant, easily led. Quiet disposition, fairly intelligent. Conduct now good.

SECTION B

J. D.	14 yrs.	11 yrs.	3 yrs.	Associated with bad companions. Habit of petty thieving.
J. R.	12½ yrs.	8 yrs.	4½ yrs.	Illegitimate. Rather untrustworthy and dull.
F. M.	13½ yrs.	9 yrs.	4½ yrs.	Truant. Fairly intelligent but of a sly disposition.
R. F.	11½ yrs.	9 yrs.	2½ yrs.	Petty thief. Rather sullen disposition.
W. P.	11½ yrs.	9 yrs.	2½ yrs.	Neglected at home. Fairly intelligent but sly disposition.
C. J.	12 yrs.	10 yrs.	2 yrs.	A truant and of plausible disposition. Fairly advanced in school.

It will be seen that the tests have brought out to a rather remarkable degree the differences in character in the boys of the two groups. The total deficiency by the tests is for Section A—3½ years; for Section B—19 years. This accounts for 13 boys out of 22. In the remaining nine cases there is nothing noteworthy in the superintendent's reports other than the references to their intelligence.

It should be stated that these personal reports must be considered thoroughly independent. None of the results of the testing were known to the superintendent at the time the reports were furnished.

Reformatory Boys

The next investigation was undertaken at the Royal Park Reformatory, Melbourne. In the light of their records the subjects must be considered as youthful criminals. Table III. summarizes their results and speaks for itself.

Table III

Passed Test	
At age	0
2 years below chron. age*	2
3 years " " "	3
4 years " " "	4
5 years " " "	3
6 years " " "	5
7 years " " "	2
8 years " " "	1
Total	20

Average chronological age	15 years 6 months
Average test passed	9 " 10 "
Average deficiency	5 " 8 "

Note.—In this table 15 years was taken as the upper limit of chronological age so as to provide a basis of comparison with the mental age. As a matter of fact the average age will be seen to have been above 15 years.

By comparison with Table I. it will be seen that, though the average age of the Reformatory boys was higher than that of the boys at the Home, they passed on the average a lower test. Some of the individual records are so interesting that I may be pardoned for giving some details.

Some of the offences charged against these lads were house-breaking (6); serious assault (causing grievous bodily harm) (3); larceny (7); obscene language (1); street gambling (1); obtaining money under false pretences (1); murder (1). The numbers in brackets here indicate the number of such offences.

The housebreakers showed on the whole better success in the tests than the others, the mental ages of the six being

12, 11, 11, 11, 10 and 8 years respectively, whilst their total deficiency amounted to 23½ years. Those who had been convicted of serious assaults had the mental ages of 11, 9, and 8 years respectively, whilst the murderer's mental age was 9 years. Those four, however, who had committed crimes of violence had a total deficiency of 23 years, an average of 5¾ years as against the average of 4 years for the housebreakers. It would seem as if the more talented in this group of youthful criminals were already qualifying for a place in the aristocracy of crime, the skilled burglars. The clumsy insensate crimes of violence were left to boys of a much lower mental level. Three of the four mentioned above were sentenced for kicking a companion nearly to death. The murderer had decapitated his victim with an axe when they were returning from a joint hunting expedition.

It is most significant that only four of this group of 20 lads were suspected by the penal authorities to be at all deficient mentally. Among these was the murderer. But the point that should be emphasized is that there were other lads of a lower mental level who were considered fully responsible. When they have committed equally atrocious crimes, their mental deficiency too may be realized.

Provided that the offenders show an amount of low cunning in the commission of their crimes the police appear very loath to admit the fact of mental deficiency. An interesting instance of this was the case of S. B., aged 15½ years, examined by me at the Melbourne Gaol. He had been committed for stealing letters during their transit from the post office to the city railway station. His plan was to represent himself to the driver of the mail van as an employee of the post office, sit at the back of the van, cut open the mail bags and abstract the contents. The police appeared very loath to agree to a theory of mental deficiency in his case and he was examined.

The highest test that he could pass in the Porteus series was that for 8 years and he required the two trials allowed for tests for 6 and 7 years. When examined per the Binet, his mental age was also eight years. He failed to tell the

differences between a butterfly and fly, etc., he could not reckon the value of the stamps nor repeat five numerals correctly. In the nine year tests he could not give change nor arrange the weights and he failed in ten year tests throughout.

The dynamometer records were 17 kilos right hand and 10 kilos left hand—very low for a youth of 16 years of age. In the Fernald-Healy construction test he required two minutes thirty seconds.

The diagnosis of feeble-mindedness was, by the foregoing results, indisputable.[1]

Aboriginal Children

For comparison with the delinquent groups I am taking the examination results of some aboriginal children examined by myself at the Mission Station, Point MacLeay, South Australia.

Few of them were full-blooded, though in many cases they were the offspring of marriages between full-blooded aborigines and half castes. Results are summarized in Table IV.

Table IV

Passed Test	
3 years above chron. age	1
2 years " " "	1
1 year " " "	4
At age	12
1 year below chron. age	7
2 years " " "	1
3 years " " "	2
Total	28

Average age	10 years	2 months
Average test passed	9 "	9 "
Average deficiency		5 "

[1] Since writing this article, I have learned that this boy was examined by a medical officer at the gaol. He was found to be syphilitic but the equally important fact of his mental deficiency was unnoticed by the doctor, or was, at any rate, unmentioned! This implies either carelessness in examination or ignorance of modern methods of psychological examination. The question of the lad's mental and moral responsibility was not raised at the trial and he was sentenced to two years' imprisonment. The diagnosis of deficiency was not disputed but ignored.

On examining the individual records it was found that the younger children succeeded, relatively, far better than the older ones. Of 12 children who were over 11 years of age, 9 are retarded by the tests, the total deficiency being 16 years. Of the 16 children under 11 years of age, 6 passed tests above their chronological age, 9 passed 'at age' and 1 was retarded one year.

The increased difficulty of the tests for the upper years is not sufficient to explain this failure of the older children. This result bears out the view that the period of mental development is, in aboriginal children, comparatively short. This also accords with the experience of the teacher of the school, who says that he finds little difficulty in bringing his pupils through the lower grades but finds it very difficult indeed to educate them beyond about the fourth grade, or midway through the school course.

Dr. Gertrude Halley, chief schools' medical officer, who examined these children physically found them well developed for their ages, weight and height being above the average. Puberty appeared to be rather early established. The prepubescent period seems to be the most favorable for mental development, but is succeeded during early adolescence by a period in which the common racial characteristics of indolence, shiftlessness, and lack of foresight become apparent. This view was strengthened by the results of the tests. It would be interesting to discover by continuing the tests whether the white race's superiority over the aboriginal is mainly due to the shorter period of mental development in the latter, force of heredity in the white enabling him better to withstand the physical strain associated with the onset of adolescence, and allowing mental development to proceed equally with that of the body.

By comparing Tables I., III. and IV. it will be seen that relatively speaking, the aboriginal children passed far better than the Reformatory boys and considerably better than the boys of Section B of Table II. By the use of these tests and of similar ones which no doubt will soon be developed, it is hoped that we may reach a stage when we can definitely state

by means of an examination that a child is abnormal not only in intelligence but in disposition and that he thus lacks the potentialities of good character forming. If, in addition, psychological tests will enable us even approximately to assess the importance of these deficiencies in their bearing on conduct, then we will have achieved a decided step forward towards the ideal of making training and education ensure the conservation of the child.

Application of Tests to Deaf Children

Interesting results were obtained by the application of the tests to the children at the school for deaf and dumb, Melbourne.

For children above nine years of age the plan adopted was to illustrate the working of the six and seven year tests, the subjects beginning with the test for eight years. It was rarely found necessary to make any further explanations or to give further instructions.

On the whole, the deaf children found the tests somewhat difficult. Temperamental peculiarities were often apparent. In some cases the subjects gave up the task, signing that it was beyond their powers. Very many showed extremely quick perceptions, and worked the tests at a great pace with a resultant tendency to error through impulsiveness. Generally they were very quick to notice their errors. A fairly large proportion were definitely feeble-minded, and a number, judged by ordinary standards, must be considered dull. In every case in which the tests showed the child to be feeble-minded the teacher's judgment concurred. As regards the boys, there was a close agreement between the teachers' estimates of intelligence and the verdict of the tests. That is to say that the tests, considered as intelligence tests, were satisfactory. In the case of the girls, however, the agreement was not so close, many failing to reach the standard that the teachers expected.

This partial failure of the tests may be explained in two ways. The first explanation is that girls on the whole do not test as high as the boys. Five hundred and eight girls

(normals) were tested in a recent investigation and their Binet ages compared with their ages per my tests. By the latter, 36 percent tested above their Binet age whilst 43 percent tested below, the remaining 20 percent testing the same. For 492 boys the corresponding figures were: Above the Binet = 44 percent; Below, = 40 percent; Same, = 15 percent.

It will be seen that girls test, on the average, lower than boys. This is probably due to greater impulsiveness and to a lesser development of foresight in girls than in boys.

The second factor influencing results is that the training of the boys at the school tends to accentuate the above-mentioned sex differences. Organized games and sport generally are made a special feature of the boys' training at the school. This is not the case with the girls. Undoubtedly the effect of this training on the boys is to make them more self-reliant, resourceful, and more mentally alert than the girls. It was noticed that the boys who played best, scored, as a rule, highest in the tests.

Notwithstanding the foregoing objections, I am convinced that the tests do enable us to make a useful estimate, within fairly correct limits, of the deaf child's native intelligence. They certainly allow him to display his ability in a much fairer light than any other tests do. Even if the Binet test is adapted to the deaf child's examination, it remains, principally, a language test, a test of comprehension. It must be admitted, however, that the successful application of the Porteus tests depends on the subject's thorough understanding of what is required of him. Once this understanding is gained the application is easy.

The dynamometer records were also taken. The right-hand and left-hand grips were added together and the ranking of the children in the tests and their ranking in individual grip records were correlated by the Spearman Footrule method.

The correlation was:

$$R = 0.65 \text{ (Boys)},$$
$$R = 0.66 \text{ (Girls)}.$$

Considering the dissimilarity existing between motor intelligence tests of a merely physical measurement, such as power of grip, I think this correlation may be considered fairly high. A noticeable feature was the large number of cases in which the grip of one hand approximated the grip of the other.

INDIVIDUAL RECORDS: DEAF AND DUMB CHILDREN

Section A

Case No.	Actual Age	Age per Test	Dynamometer			Case No.	Actual Age	Age per Test	Dynamometer		
			R.	L.	Total				R.	L.	Total
1	$17^{8}/_{12}$	13	47	48	95	19	$9^{1}/_{12}$	9	21	21	42
2	$17^{6}/_{12}$	13	45	48	93	20	$11^{7}/_{12}$	12	20	17	37
3	$17^{7}/_{12}$	12	46	43	89	21	$10^{10}/_{12}$	10	18	18	36
4	$17^{3}/_{12}$	12	43	44	87	22	$9^{8}/_{12}$	10	20	16	36
5	$15^{3}/_{12}$	13	40	37	77	23	$8^{3}/_{12}$	10	17	16	33
6	$16^{10}/_{12}$	13	38	36	74	24	$11^{4}/_{12}$	11	17	16	33
7	17	11	32	42	74	25	$9^{10}/_{12}$	8	16	16	32
8	$14^{8}/_{12}$	11	37	36	73	26	$11^{10}/_{12}$	8	17	15	32
9	$17^{10}/_{12}$	12	33	38	71	27	$10^{8}/_{12}$	11	14	14	28
10	$16^{3}/_{12}$	11	32	37	69	28	$12^{2}/_{12}$	9	14	13	27
11	$15^{6}/_{12}$	$11^{6}/_{12}$	35	33	68	29	$10^{9}/_{12}$	10	13	12	25
12	$16^{10}/_{12}$	$11^{6}/_{12}$	34	31	65	30	$8^{6}/_{12}$	11	13	12	25
13	$15^{5}/_{12}$	13	26	28	54	31	9	9	13	10	23
14	$12^{5}/_{12}$	10	26	24	50	34	$10^{10}/_{12}$	10	10	11	21
15	$16^{1}/_{12}$	13	26	23	49	35	$7^{3}/_{12}$	8	11	10	21
16	$13^{9}/_{12}$	12	23	21	44	36	$6^{11}/_{12}$	7	8	7	15
17	$13^{5}/_{12}$	11	20	23	43	37	$6^{2}/_{12}$	7	7	8	15
18	$12^{9}/_{12}$	12	23	20	43	38	$6^{8}/_{12}$	$6^{6}/_{12}$	5	7	12

Section B

Mentally Deficient

Case No.	Actual Age	Age per Test	Dynamometer			Case No.	Actual Age	Age per Test	Dynamometer		
			R.	L.	Total				R.	L.	Total
1	$16^{2}/_{12}$	10	35	28	63	7	$11^{2}/_{12}$	8	13	13	26
2	$14^{10}/_{12}$	9	31	26	57	8	14	7	16	10	26
3	19	11	23	27	50	9	$9^{5}/_{12}$	6	12	14	26
4	$15^{11}/_{12}$	10	25	24	49	10	$11^{3}/_{12}$	7	11	10	21
5	$14^{4}/_{12}$	8	19	20	39	11	$11^{3}/_{12}$	7	9	9	18
6	$13^{3}/_{12}$	9	19	20	39	12	$8^{8}/_{12}$	4	5	5	10

A close approach to ambidexterity appeared in about sixty percent of cases, a difference of less than four kilos. in strength in each hand appearing in that proportion. In this particular the deaf appear to resemble closely the mentally deficient, a recent investigation by the author amongst feeble-minded revealing a similar condition. It may be said

of the latter that they are generally equally weak in each hand, while of the normal deaf it may be said that they are, generally speaking, equally strong in each hand.

In the following summary the normal boys' results have been separated from those of the boys who were considered feeble-minded. In the table showing individual records the figures for the latter are given in Section *B*.

Table V.

Normal Boys

Average age ..... $12^{7}/_{12}$ years
Average test passed ..... $10^{7}/_{12}$ years
Average deficiency ..... 2 years

Feebleminded Boys

Average age ..... $13^{2}/_{12}$ years
Average test passed ..... 8 years
Average deficiency ..... $5^{2}/_{12}$ years

Girls

Average age ..... $11^{6}/_{12}$ years
Average test passed ..... $9^{5}/_{12}$ years
Average deficiency ..... $2^{1}/_{12}$ years

Individual Records: Girls

Case No.	Actual Age	Age per Test	Dynamometer			Case No.	Actual Age	Age per Test	Dynamometer		
			R.	L.	Total				R.	L.	Total
1	$14^{5}/_{12}$	10	34	26	60	15	$12^{1}/_{12}$	9	16	13	29
2	$13^{10}/_{12}$	11	26	25	51	16	$11^{5}/_{12}$	10	15	13	28
3	$13^{4}/_{12}$	12	25	23	48	17	$9^{8}/_{12}$	10	15	12	27
4	$12^{5}/_{12}$	10	25	23	48	18	$12^{8}/_{12}$	8	14	13	27
5	$17^{6}/_{12}$	12	24	23	47	19	$12^{10}/_{12}$	11	13	12	25
6	$15^{10}/_{12}$	11	25	21	46	20	$8^{2}/_{12}$	8	13	10	23
7	13	13	22	20	42	21	$8^{3}/_{12}$	8	10	9	19
8	$13^{5}/_{12}$	9	21	19	40	22	$6^{8}/_{12}$	6	10	9	19
9	$13^{11}/_{12}$	11	20	18	38	23	$7^{1}/_{12}$	6	10	9	19
10	$12^{9}/_{12}$	12	18	19	37	24	$9^{1}/_{12}$	7	12	6	18
11	$13^{9}/_{12}$	11	20	15	35	25	$6^{4}/_{12}$	6	8	8	16
12	$12^{1}/_{12}$	10	18	14	32	26	$5^{1}/_{12}$	5	6	6	12
13	$15^{9}/_{12}$	9	16	16	32	27	$7^{3}/_{12}$	$8^{6}/_{12}$	11	11	22
14	$13^{10}/_{12}$	11	18	13	31						

In each of the above investigations the number of individuals examined is certainly small. The results are reported in order to show the applicability of the tests to abnormal children generally. Results with mentally deficient and with normal children have been elsewhere reported.

THE PSYCHOLOGICAL CONCEPT OF CLEARNESS

BY E. B. TITCHENER

In 1913 C. A. Britz published a thesis for the Zurich doctorate entitled *Eine theoretische und experimentelle Untersuchung über den psychologischen Begriff der Klarheit.* Circumstances over which I have no control postponed my first-hand acquaintance with the work to 1916. I have regretted this the more because Britz deals in detail with two psychological systems in which the notion of clearness holds a prominent place, Wundt's and my own. Wundt he chose for obvious reasons; myself, because my 'Standpunkt stellt quasi ein Extrem dar.' Both of us receive a severe mauling: which would be wholesome enough—since the criticism is objective and based upon quotation—if only Britz had followed a sound method. He has not. He struggles with the Wundtian concepts of *clearness* and *degree of consciousness* on the basis of the sixth edition of the 'Physiologische Psychologie' and the seventh of the 'Grundriss'; and it never occurs to him that the key to their understanding is a genetic study of his author. He attacks my concept of sensory clearness on the basis of my 'Text-book' and of Hillebrand's review of my 'Feeling and Attention' in the *Zeitschrift;* he has not referred, incredible as the thing appears, to the *Feeling and Attention* itself.

External circumstances may be in part responsible. The thesis was undertaken at Schumann's suggestion, and the experimental portion was apparently completed under his direction at Frankfurt. The author, however, returned for his doctorate to his old university, and the thesis was accepted by G. F. Lipps of Zurich. It is a fair assumption (is it not?) that Schumann was chiefly interested in the experiments, and that Lipps was generously disposed to a bit of work originated and approved by a psychological colleague elsewhere. In that case the critical chapters, about seventy per-

cent of the whole paper, would have fallen, so to say, between the two professorial chairs. Even so it is astonishing that Britz should not have learned for himself the essentials of scientific method.

I

I do not imagine that Wundt will find time to defend his system against these latest charges, and I do not propose to undertake the business for him. I shall, however, try to set forth, in the light of a genetic study, the use and meaning of the technical terms here in question. Such a study is doubly instructive. It shows our modern psychology in the making; the long series of Wundt's books reflects the recent history of the science. It shows also the manner of Wundt's own progress from logic to psychology, from activity to content. His fundamental ideas have remained, for the most part, unchanged; advance is made, habitually, by modification in detail, by expansion and contraction, by redistribution of topics and change of emphasis. Even when the system suffers a decided innovation (we shall have a case presently), there are always hints of the new departure, if we look closely enough, in the previous work. Here, of course, I have space only to give results. I hope to be able, nevertheless, to clear up the difficulties which Britz and, perhaps, other readers have found in the sixth edition of the 'Physiologische Psychologie.'

We are to ask, accordingly, what Wundt means by consciousness and degrees of consciousness, by clearness, by degrees of apperception, and especially what is the relation between degree of consciousness and degree of clearness. We begin with consciousness.

The keynote of Wundt's psychological treatment of consciousness is the notion of *synthesis*. Consciousness, the condition of all inner experience (1–5)[1] or, more empirically, the total contents of our immediate experience (6), cannot be defined in psychological terms. We must be satisfied to determine its conditions (1), that is, the phenomena that

[1] By these numbers I indicate the editions in which the particular phrase occurs; minor verbal changes must here be disregarded. References are given in later notes.

invariably accompany its manifestation in experience (2–5); more exactly, we must be content to give the conditions under which we observe such phenomena as we attribute to a consciousness (6). There are, now, two psychological processes which are bound up with consciousness and may be regarded as its essential characters. The first of these is the formation of ideas (and real feelings) from sensations (and simple feelings). Our consciousness of ideas consists in the *act of synthesis* whereby sensations are brought into temporal and spatial form (1); in every act of ideation there is effected a connection of elementary sensations (2–4). Ideas and real feelings arise from a psychological synthesis of elements, and this *connection of elements* is therefore one of the two characteristics of consciousness (5, 6). The other is to be found in the processes of reproduction and association of ideas (and feelings). The connection of ideas takes place *in* consciousness (1); it is only by way of reproduction and association that consciousness can become aware of itself as persisting without change through all the change of ideas (1–3); this changing flow of ideas is itself aware of consciousness as a *synthetic activity* connecting present ideas with those that have gone before (1–3). Consciousness is empirically demonstrable only on condition (*unter der Voraussetzung*) of a connection of the ideas (and feelings) which follow one another in time (4, 5); reproduction and association are therefore an universal concomitant of consciousness (6). An orderly connection of ideas (1–4) or, in more general phrase, a connection of immediate experiences (5, 6) is, indeed, the condition under which alone consciousness appears.

If, however, the primary thing about consciousness is synthesis, then we must recognize the possibility of *degrees* or grades of consciousness, since such connections as that of sensations in the temporal or spatial idea may exist at various levels (1). Self-observation reveals these degrees. "Whenever we incorporate an impression but loosely in the context of our ideas, or later remember it but imperfectly by reason of this looseness of connection, we credit ourselves only with a lower degree of consciousness at the time in question" (2–6).

In these instances, capacity for the connection of ideas (or psychical contents) is taken as measure of degree of consciousness (2–5); or, as Wundt puts it in his final phrasing, "the *connection* of psychical contents is a certain measure of degree of consciousness" (6). Every connection of inner states (or psychical elements) manifests some degree of consciousness (2–6).

That is the Wundtian doctrine of consciousness and its degrees, as set forth in the various editions of his great work.[1] The intention of the discussion, from the very first, is psychological; even in 1874 Wundt is combating the logical tendencies which showed their full force in the *Vorlesungen* of 1863.[2] His effort is not immediately successful; but, by degrees, changing here a little and there a little, he moves away from logic toward psychology, away from synthetic activity toward observable connection. There is no reason to think that he was, at every stage, fully aware of the significance of the changes; he probably chose the wording that seemed, at the time, best to express his thought—his original no less than his present thought. In point of fact the corrections of the early text serve in sum to change the whole atmosphere of the discussion.

We find the same sort of progress in the treatment of attention. The discussion of 1874 begins as follows: "In the synthesis of sensations and in the association of ideas consciousness apprehends itself as active. Thus arises that expression of consciousness which we name *attention*. It makes itself known in direct self-observation by the fact that the interconnection of ideas, with which consciousness is correlated, is by no means present to it at all times in the same manner; consciousness is directed upon certain ideas in higher measure than upon others." Later we have: "Beside the coming and going of ideas we perceive within us not infrequently (in varying fashion) and more or less plainly an (inner) activity which we designate *attention*" (2–6).

[1] *Phys. Psych.*, 1874, 707 f., 711 ff., 717; 1880, II., 195 f., 199, 201; 1887, II., 225 f., 229, 231; 1893, II., 255 f., 259, 261; 1903, III., 320 f., 324 f.; 1911, III., 296, f.; 299 f.

[2] See *op. cit.*, 1874, 708 ff.

Degree of apperception is gauged by the subjective activity with which consciousness turns to a particular sensory stimulus (1–5). The simile of *Blickpunkt* and *Blickfeld*, the statement that the *Punkt* is really a small *Feld* of varying extent and that the main field darkens in proportion as the central field brightens, and the distinction of perception and apperception are present in all editions. Passive and active apperception are distinguished in the second, the limen of consciousness and the limen of attention only in the fourth edition. All these things are familiar, and need not be dwelt upon. What now of clearness, which ultimately becomes the sole objective criterion of apperception?

The brightening of the *Blickpunkt* means, as we have seen, that consciousness is directed upon certain ideas in higher measure than upon others (1–4); certain contents become *more conscious* than others (5); we observe in consciousness *different degrees of conscious status*, variously and varyingly distributed over its contents (6). If we consider the apperceived contents themselves, we find the following progression. We begin with a clearness of ideas, dependent partly on the intensity of the ideas and partly on adaptation of attention (1–3). Presently this clearness, dependent now upon the intensity of the sensations composing the ideas and upon adaptation of attention, is paired with distinctness; clearness is predicable of an idea in its own right, distinctness of an idea in its relation to other ideas. Feelings may be distinct, but apparently can not be clear (4). Later still this same clearness attaches to all complex conscious contents: to ideas and feelings as wholes, and also to particular elements within ideas and feelings (5, 6). Clearness, which originally belonged to ideas alone, thus remains to the end a character of complex contents. "Clearness and distinctness are exclusively characters of ideas, and may be transferred to sensations only when these are considered as constituents of ideas" (6).[1]

All this is fairly straightforward, though I must warn the

[1] *Op. cit.*, 1874, 717 f., 720, 722, 725, 729; 1880, II., 205 f., 208, 209, 212; 1887, II., 235 f., 238, 239, 244; 1893, II., 266, 267, 269, 271, 272, 282; 1903, III., 331, 332 f., 336, 337 f., 339, 348, 349; 1911, III., 306, 307 f., 312 f., 314, 322, 323.

reader that I have passed over certain passages which will occupy us later. The pairing of distinctness with clearness offers no difficulty; distinctness is always the subordinate concept, and does not appear in the final summary of the part-processes in an apperception.[1] The irruption of the new theory of feeling, in the fifth edition, does create a difficulty—as I pointed out in *Feeling and Attention*—but it is not one that directly concerns us here.

We are therefore ready to take up, in a preliminary way, the relation of consciousness to attention and of degree of consciousness to degree of clearness. The first of these questions is easily answered. Consciousness, for Wundt, is always wider than attention. In 1874 he wrote: "The theory that consciousness and attention are identical is not tenable." In 1911 he writes: "An impression that has sunk below the limen of apperception does not therewith disappear from consciousness;" and his whole treatment of the two topics, from first to last, implies this distinction.[2] Consciousness is the total contents of our immediate experience; attention is the range of clear experience.

The second question may be answered, to begin with, by the statement that degree of consciousness and degree of clearness have, logically, no connection with each other. Degree of consciousness is degree of organization of conscious contents. Let us imagine (if we can) a consciousness without attention. Such a consciousness would still show degrees of consciousness, because the complex contents and the groups of complex contents which make it up would differ in closeness of connection or organization. There are passages in the first edition which seem to come very near to such an imaginary consciousness;[3] and the recurring phrase "It is always association that puts ideas at the disposal of apperception" at any rate suggests it.[4] In reality, however,

[1] See *op. cit.*, 1911, III., 316.

[2] *Op. cit.*, 1874, 725; 1911, III., 314.

[3] *Op. cit.*, 1874, 795, 835.

[4] *Op. cit.*, 1880, II., 212; 1887, II., 244; 1893, II., 279. I do not find the phrase in the two last editions; and indeed it goes too far. See 1903, III., 524 f.; 1911, III., 498 f.

consciousness comes to us in other guise. It makes its own history from the very outset, and carries that history with it; and the history is constantly interfering, so to say, with its present course.[1] Or, to put the same thing from another point of view, it is organized, at whatever level and in whatever degree, as an attentive consciousness; associations are formed in passive, apperceptive connections in active attention.[2] While, then, degree of consciousness (or of organization) and degree of clearness (or of conscious status) may be distinguished logically, empirically they are bound together in the most complicated fashion. One might suppose, perhaps, that the difference between range of consciousness and range of attention should be directly observable. Wundt does not deny it, though he seems to think otherwise; it is a subsequent apperception that ordinarily makes us aware of the obscure fringe; and he does deny that the simultaneous method is adequate to range of consciousness.[3] One might suppose, again, that degree of consciousness and degree of clearness should run parallel; and in many cases, at many moments of the history of consciousness, they doubtless do; but we must remember that the one tends to be stable and the other is essentially instable. Consider, indeed, any case of active attention. The complex contents that now lie in the obscurity of the *Blickfeld*, and that therefore have no conscious status whatever, were once (probably, many times over) given in passive apperception, in so far as they are organized at all; and if their organization is high, as it may be, they were given in active apperception. The complex contents that occupy the *Blickpunkt* and therefore possess various degrees of conscious status vary in degree of consciousness, from moment to moment, according as apperception is integrative or disruptive and their organization is correspondingly strengthened or weakened. Or consider observation itself. Observation is always apperception; and we cannot become aware of a low degree of consciousness unless

[1] *Op. cit.*, 1893, II., 284.

[2] See W. B. Pillsbury, *Amer. J. of Psychol.*, 1897, **8**, 329 ff.

[3] *Op. cit.*, 1911, III., 324, 330; cf. 1903, III., 351 ff., and the stronger statements of 1874, 726; 1880, II., 219; 1887, II., 261; 1893, II., 305.

we give the poorly organized contents a high conscious status.[1] So the empirical relation of the two degrees (and I have, of course, greatly oversimplified it in this brief account) is complex in the extreme. Neither can exist without the other; yet, since they do not run on parallel lines, their separate treatment is a matter of practical convenience, if not of necessity; a full account, historical and descriptive, of any given consciousness implies constant reference to both. If Wundt inclines, even in his latest writing, to make connection the fundamental character of consciousness and to regard attention as an activity within consciousness[2]—when we might expect him to give the two factors equal rank—the reasons are historical, and not least among them is his reaction against unconscious ideas.

I believe that these answers to our two questions are fair, and that they represent the essentials of Wundt's doctrine; I confess that I have rounded off some rather prickly passages.[3] We have now to consider those divergent statements to which I have already referred.

[1] After a good deal of vacillation, Wundt settles down in the sixth edition to the definite terminological distinction of *Bewusstseinsstufe* or *Grad des Bewusstseins* and *Grad der Bewusstheit*: 1911, III., 299, 307. I have made this distinction throughout, and have translated *Bewusstheit* by 'conscious status.'

[2] *Op. cit.*, 1911, III., 301.

[3] Let me give an instance! Wundt teaches that the contents at the *Blickpunkt* (an area, be it remembered) are variously clear, and the contents in the outlying *Blickfeld* obscure. In 1903, III., 353 (1911, III., 326) we are introduced to *Grade der Verdunkelung*, degrees of obscurity. It looks, then, as if the contents below the limen of attention might possess something more than degree of consciousness (which is all that I have allowed them in the text), something that is, after all, very like conscious status. I have, however, pointed out in 'Feeling and Attention' that there is here a confusion of apperception with cognition, of attributive with cognitive clearness, and that a recent worker in Wundt's own laboratory has called attention to it (see 237 ff., 369, and cf. 230 f.). I have already remarked that Wundt's progress is from activity to content: it is not till the fifth edition that 'consciousness' ceases to be 'directed upon ideas,' and that 'contents become more conscious': cf. 1903, III., 333 with 1893, II., 267.

Again, in 1902, I., 323 (1908, I., 382) we are told that change of clearness, as distinguished from change of intensity, alters the relation between contents; clearness thus seems to be confused with distinctness. But the passage in which this statement occurs is not represented in 1893; it harks back to 1887, I., 237, an edition in which the distinction of clearness and distinctness had not yet been drawn. The clearness of 1902 and 1908 (in these particular sentences) is therefore an undifferentiated clearness *and* distinctness.

The discussion of the apperception-center in the fifth edition surprises us by a reference to the clearness of *sensation:* surprises us all the more because the earlier editions spoke in the same context only of the clearness of ideas and impressions; because the same volume teaches that sensations are constituted solely of intensity and quality; and because the same edition, in a later volume, retains the orthodox view that clearness and distinctness are exclusively characters of ideas. This third volume adds, however, that clearness may be predicated of sensations when they are considered as constituents of ideas (the fundamental of a compound tone, the color of a visual form); and so it seemed possible to interpret the sensations which become clear in apperception as sensations-in-ideas.[1] That was a way out of the difficulty; it was not the way the offending passage read. Those who know the 'Physiologische Psychologie' historically know, however, that its exposition is continually changing in detail, and that the details are likely to prove important; I have made the point earlier in this paper. So one hoped for more light in a sixth edition; and the light came with a vengeance! The reference to clearness of sensations in the discussion of the apperception-center is now justified by entirely new matter, which introduces the chapter on Intensity of Sensation. There are (we learn) intensive psychical magnitudes, which accrue only to the simple elements of the mental life, and there are extensive psychical magnitudes, which result from the composition of elements. The three intensive magnitudes are intensity, quality, and—clearness. And these three characters are three coördinate dimensions of the psychical elements; they are, that is to say, *attributes of sensation.* Compound contents show different degrees of clearness in their different parts; degree of clearness is unequivocal

[1] *Op. cit.*, 1902, I., 322 f., 353; 1903, III., 338, 349; cf. 1880, I., 218; 1887, I., 233; 1893, I., 228. These passages are to be sharply distinguished from the casual and physiologically motived reference to the apperception of sensations (correlates of the excitation of a sensory center) which occurs in 1902, I., 324, and which appears in all editions from the second to the sixth.

only in regard to the elements, to simple contents. Wundt's statements are as definite as they could well be.[1]

Here, then, is a new kind of clearness, different from the original clearness of ideas and real feelings, different also from the clearness of the sensations-in-ideas. Wundt has not made a clean sweep of things, however, even in this volume; the old statement, running through all the editions, that sensations are constituted solely of intensity and quality,[2] is allowed to remain. A pure oversight, no doubt! The few references to clearness in the second volume, of 1910, are neutral. But then we come to the third volume, of 1911, and there we are back again in the familiar atmosphere, with the express assurance that clearness and distinctness are exclusively characters of ideas! Could ever anything be more bewildering?

Well! our bewilderment is at any rate less than Britz's. For our genetic study proves that the two clearnesses do not stand on a level. The clearness of the third volume, of 1911, is the traditional clearness of the Wundtian system, deeply rooted in nearly forty years of thought and expression; the clearness of the first volume, of 1908, is a new phenomenon, only casually foreshadowed in the corresponding volume of 1902. Something, it appears, was moving Wundt's ideas, even at the earlier date, towards sensory clearness; and something happened, between the fifth and sixth editions, to precipitate and crystallize his ideas.[3] Thereafter, in the interval between 1908 and 1911, his interests turned away from this something; he had forgotten all about his intensive

[1] *Op. cit.*, III., 1908, I., 539 ff. I myself urged as early as 1898 that clearness should be recognized as a sensory attribute, but printed no extended discussion of the question before this same year, 1908. See *Phil. Rev.*, 8, 461 f.; 'Feeling and Attention,' references to 'Clearness' in index.

[2] Together (for a time) with feeling-tone. See 1874, 273; 1880, I., 272; 1887, I., 290; 1893, I., 282; 1902, I., 353; 1908, I., 412.

[3] I am in this paper expounding Wundt, and neither criticizing his views nor trying at all completely to trace their motivation; the first thing to do with an author (and it is what Britz has failed to do) is to understand him. I think, however, that it is safe to connect Wundt's new paragraphs with the revival of psychophysical interest shown by the works of Müller (1904), Lipps (1903, 1904, 1906), Titchener (1905), Bruns (1906), Keller (1907) and others. In particular, G. F. Lipps was at Leipsic from 1903 on.

magnitudes that accrue only to the elements; and so he contented himself with the customary revision of the former text. Natural enough, after all, in view of Wundt's age and multifarious activities: the wonder is not so much that he should have forgotten as that he should have had, in 1908, the energy and the open-mindedness to attack once again the whole problem of mental measurement, and in doing this to effect a radical change in one of his most elaborate systematic constructions. There is no possibility of reconciliation of the two volumes; Britz's efforts are wasted labor.[1]

It would plainly be useless to reopen our questions of the relation of consciousness to attention, and of degree of consciousness to conscious status, in the light of Wundt's new definitions. The clearness which is an intensive attribute of sensation is at the same time degree of apperception (the objective aspect of degree of attention) or of keenness of apprehension.[2] We may work out, if we will, what this statement logically implies for the treatment of consciousness and attention in the third volume; or we may wait patiently for a seventh edition. At present the questions can be answered intelligibly only if we ignore the intruding passages. They can be answered, that is, only in the preliminary way in which they have been answered above. This conclusion seems to me to be a positive result, which justifies our recourse to the genetic or historical method. It far outweighs, just because it is the result of a sound method, any conclusion reached by Britz.

II

I must now say something in reply to Britz's polemic against my own doctrine of clearness. If only Britz knew accurately what he is talking about! I give a few examples to show that he does not.

(1) Britz finds five principal "criteria" of the sensory attribute. I am said to rely only upon two, inseparability

[1] But what was Klemm about, who read the proofs, that he did not call his chief's attention to the discrepancy? See *op. cit.*, 1908, I., X; 1911, III., V.

[2] *Op. cit.*, 1908, III., 541 ff. The italicized *Apperzeptionsgrade* of p. 541, l. 16 should be *Aufmerksamkeitsgrade*.

and independent variability; and I am further said to rely mainly upon the second. Turn to 'Feeling and Attention'! I there begin by criticizing the 'common definition' of an attribute. From this definition I accept the criterion of *inseparability*. As a mark of inseparability I instance the reduction of the whole sensation to zero when a single attribute reduces to zero. Britz, who raises this special case of inseparability to the rank of a separate criterion, speculates as to what I should make of the argument if I used it! From the same definition I get the criterion of *independent variability*, and point out that in fact "there are bound attributes as well as free," so that "the test of independent variability, useful enough for a preliminary survey, must be applied with caution when we demand accuracy of detail." Yet this is 'Titchener's Hauptmerkmal'! Hillebrand, from whom Britz derives my two criteria, states the case correctly; so that Britz has even misread Hillebrand.[1]

(2) Britz does not hesitate to criticize Bentley's experiment with intensities of sound on the basis of the passing reference in my 'Text-book.' What series were carried out, what intensities of stimulus were employed, what precautions were taken, of course he does not know.[2]

(3) I never use the phrase 'degree of consciousness'; from my point of view it is as nonsensical as 'degree of matter' or 'degree of material existence' would be in physics. Britz disregards my definition of consciousness, and his discussion of the place of *Bewusstseinsgrad* in my system is consequently all in the air.[3]

(4) Britz credits me with 'the assumption that the number of degrees of clearness is not the same for all departments of sense.' What I say is that we have 'to determine, introspectively, how many degrees of clearness can be distinguished [how many just noticeable differences of clearness there are]

[1] Britz, *op. cit.*, 14, 24; 'Feeling and Attention,' 8 ff.; F. Hillebrand, *Z. f. Psych.*, 1910, 58, 141.

[2] *Op. cit.*, 26; 'Text-book,' 1910, 280; 'Feeling and Attention,' 361 ff. I take this opportunity to correct a misprint. In Table II., p. 364, the second rubric under Height of Fall should be 74.4–89.6 cm.

[3] *Op. cit.*, 41 f.

in the various departments of sense'; I assume neither that the number is the same nor that it is different.[1]

(5) I have just said that Britz misreads Hillebrand: here is another case. "Hillebrand has shown in detail," he writes, "to what absurdities we are led by the identification of attention and clearness when we make clearness an attribute of sensation." Hillebrand has shown no such thing. He raises the question "whether every attribute has its own clearness, as an attribute of the second order"; if it has, he says, "that would lead to the absurd consequence that the complete disregard of any one attribute brought with it, to say the least, the disregard of all the others. . . . Here Titchener seems to me to have overlooked obvious difficulties." Perfectly fair criticism! but there is one absurdity hinging on an 'if,' not an *ausführliche Darlegung* of absurdities in general.[2]

It is naturally disappointing, when one is made the part-subject of a doctorate thesis, to find one's views thus caricatured. But enough has been said on that matter. Let us now see if Britz makes any positive contribution to the discussion of sensory clearness.

My thesis is that clearness or vividness (I am not yet sure which is the better term, and there is historical warrant for both) is one of the intensive attributes of sensation. Britz complains that I say very little about its actual nature; and in a sense that is true. You cannot say much about a thing that you regard as ultimate to your science; any attempt at a definition runs over, by force of circumstances, into what Wundt would call a *tautologische Umschreibung*. I have had recourse to a number of these periphrases; but I have tried, above all, to exhibit the thing itself, to state conditions under which it may be experienced and identified in experience. Quality and intensity are here in the same box with clearness. You can exhibit qualities or intensities, as you can exhibit clearnesses; but when you attempt to define them, you find yourself talking round them. If Britz had performed Geiss-

[1] *Op. cit.*, 41, 42; more correctly stated on p. 12; 'Feeling and Attention,' 277 f.; 'Text-book,' 295 f.

[2] *Op. cit.*, 42; Hillebrand, 146 f.

ler's simple experiment with the two metronomes, equated for quality and intensity of sound, he would have discovered at first hand what I mean by sensory or attributive clearness.[1]

My thesis is, secondly, that sensory clearness is the elementary phenomenon in what is ordinarily called attention. Just as sensory extension is the elementary phenomenon in spatial perception, and sensory duration in temporal perception, just so, *mutatis mutandis*, is sensory clearness the unique thing, the psychologically ultimate thing, in attention. Hence I remark in the 'Text-book' that "in the last resort, and in its simplest terms, attention is identical with sensory clearness." Analyze an attentive consciousness, and everything is familiar to you but the one thing, which you finally arrive at—this sensory clearness or vividness; that is new and characteristic.

The importance of such a view for experimental psychology is, I think, plain on the surface; a new road is opened, and a road that by all analogy should take us an appreciable distance to our goal, for an experimental attack upon attention. In 'Feeling and Attention' I speak accordingly of a 'simplified' or 'elementary' psychology of attention; I suggest that we start out, not from the gross facts of the attentive consciousness, but from the 'rise' of the single sensation, the absolute temporal limen, the carrying power of clearness under simple conditions. "How far this elementary psychology of attention could be carried it is, evidently, impossible to predict," though there is no lack of specific problems; in any case, "the results of experiment in these fields must be 'interpreted' by a psychology of attention; the factors that make for clearness must be separated from the other conditions involved, and must if possible be separately estimated or 'weighted.'" That is my view; and I am correspondingly surprised to find Britz, who quotes correctly the sentence from the 'Text-book' given above, asserting in several places that I identify outright clearness with attention. If that were the case my

[1] *Op. cit.*, 40, 41, 44; 'Text-book,' 53, 279; 'Feeling and Attention,' 26, 183 ff.; L. R. Geissler, in *Amer. J. of Psych.*, 1909, 20, 510. Britz devotes a special section to Geissler, as he does also to Wirth and Jaensch; all three will, I expect, find something to say for themselves.

chapter on Attention would hardly have been written as it is. But having made this identification, it is easy for my critic to show that the introductory examples of attention—the shift of interest due to the visit of a friend or to the receipt of a telephone message—involve more than sensory clearness, and that I am therefore faithless to my theory before I have got it formulated.[1]

I hold, thirdly, that clearness is not an attribute of the simple feeling; and as clearness is an intensive attribute, ranging from liminal obscurity to terminal clearness (just as intensity ranges from the very weak to the very strong), this means that feeling is for me neither clear nor obscure, but only qualitative, intensive and durative. The traditional 'obscurity' of feeling rests, I believe, upon the customary mixture of logic and psychology. I realize that the whole psychology of feeling is debatable ground; but, after all, the discussion in 'Feeling and Attention' is seriously written and deserves to be taken seriously. Britz gives a single sentence to the matter. We ought, he says, to enquire carefully whether the clearness of my two examples (the friend's visit and the telephone message) is not applicable to feeling; "nach meinem Dafürhalten kann er [der Begriff] angewendet werden." But *that* clearness is evidently cognitive as well as attributive; the distinction is clearly drawn, again, in 'Feeling and Attention.'[2]

I hold, lastly, that in cross-section the attentive consciousness is arranged, for many and perhaps for most of us, at two main levels, the upper of which certainly, and the lower probably, are 'wrinkled' by minor differences of sensory clearness. "A two-level type," Britz remarks, "seems to me to be altogether beyond the range of psychological proof; it too obviously contradicts all and every experience." Oddly enough, it seems to me to *represent* my experience. Britz may very well belong to the multi-level type, though he does not appear to have gone beyond casual self-observation. He continues: "Within the apperceived

[1] *Op. cit.*, 12, 40 ff.; 'Text-book,' 266 f.; 'Feeling and Attention,' 209 f., 251, 372.
[2] *Op. cit.*, 42; 'Feeling and Attention,' 237 ff.

(*beachteten*) complex there are degrees of clearness which may be lower than the highest upon the low level; therefore it is not permissible to speak of a 'niveau,' 'level,' plane or surface." I do not know how he gets his data; but it is surely plain that, if there are *anywhere* in a given consciousness processes less clear than the clearest of the lower level, these processes must be for me *at* the lower level. To say that they are 'within the apperceived complex' means, if it means anything, that my critic is thinking of the unitary object of attention; and that means that he has fallen into a form of the stimulus-error.

Hillebrand's comment here is more to the point. He asks why, if clearness is an intensive attribute of sensation, there should be only two main levels of clearness at any moment instead of an unbroken section of the attributive continuum. I do not know, though I might if I knew more physiology. I do not know either why the constant of attention is 6; our theories of attention are still nothing better than more or less plausible hypotheses. I am trying only to ascertain the psychological facts.[1]

We are not reaping an abundant harvest. Nor shall we fare much better if we ask, as I now proceed to do, why Britz objects to clearness as an attribute of sensation. He first examines the criterion of inseparability, and finds that the attribute of sensation is not separable by sensory attention but is, of course, separable by abstraction. Then 'as regards clearness' he adds: "not every phenomenon that is inseparable by sensory attention is thereby given immediate status as attribute of sensation." This statement, in the absence of examples, is a little cryptic; Britz may be thinking of some form of 'inseparable association.' We need not guess, however, since the conclusion is simply a *non liquet*. He asks, secondly, whether the sensation disappears as a whole when clearness becomes zero, and replies that, for Wundt, it does not; Wundt has two limens, the one of consciousness and the other of attention. How this reply bears upon my position I do not understand; nor, by his own admission, does

[1] *Op. cit.*, 53; Hillebrand, 148.

Britz. He examines, thirdly, the criterion of independent variability, and finds that clearness is not an independent variable. He forgets (though Hillebrand had told him) that I recognize bound attributes as well as free. He asks, fourthly, whether a reference to clearness is necessary to the complete description of a sensation, and decides that it is not. "Can we characterize a sensation completely without recourse to the notion of clearness? I must answer this question in the affirmative." Yes, but he does not show us in a concrete case how the thing may be done. Further: a supposed attribute may prove to be analyzable into a number of really primary attributes. Yes, and I have been on my guard; witness my treatment of *Aufdringlichkeit* and of tonal quality. Britz quotes no cases. Further: very few psychologists have regarded clearness as an attribute of sensation. Yes, again: and how has attention fared in the history of psychology? Listen to Ebbinghaus: "Attention is a real perplexity in psychology. Both in the general run of English associationism and in certain comprehensive works down to the present day it is altogether ignored. In other books it bears the strangest relation to the systematic presentation of the whole subject, and sometimes an author seems to be entirely helpless." And when psychology came to deal with sensations? I quote Ebbinghaus once more. "All statements of any exactness regarding sensations, their attributes, their liminal values, etc., imply from first to last—as everybody always understands without being specially told—that a high degree of attention was given to them." In other words: so long as psychology dealt with the full attentive consciousness, we made no solid progress; and when psychology acquired methods of precision, attention was taken for granted. Now that a suggestion for the beginnings of an exact psychology of attention are forthcoming, one would think they were worth a trial. Very few psychologists have agreed, as a matter of fact, upon *any* general view of attention.[1]

There remains, fifthly, the empirical side of an issue al-

[1] *Op. cit.*, 23, 24, 30, 31; 'Text-book,' 54 f., 95; 'Feeling and Attention,' 26 f., 326 f.; H. Ebbinghaus, 'Grundzüge der Psychologie,' 1902, I., 585 f., 588.

ready raised theoretically. *Is* clearness analyzable and not simple, derivative and not primary? Britz replies, on the ground of critical discussion and of experimental work, with an emphatic Yes. Clearness (both Wundt's and mine, apparently) is a very mixed concept, deriving partly from the metaphysical philosophy of Leibniz, partly from popular psychology (we talk of 'clear' colors as we talk of 'pure' tones), and partly from the properly psychological distinction of degrees of consciousness; it may thus be very variously employed, under variòus empirical conditions, and its employment always implies a process which is of the nature of judgment.

I have, now, said something of the value of Britz's critical discussion, and I could say a good deal of the value of his experiments. He worked with the tachistoscope (not knowing, of course, what I had said of that in 'Feeling and Attention,' though he had read Mittenzwey); he required his observers to cognize and name (*erkennen und benennen*) the colors exposed; and he employed a *wissentliches Verfahren* to the extent, at any rate, that they knew the nature of his problem. What the tachistoscopic analysis of the *Erkennungsvorgang* has to do, in any direct way, with the study of attributive clearness, it is difficult to see.[1] I shall not, however, enter into detailed criticism; experiment is best met by experiment; and while a repetition of Britz's work will hardly help us to a psychology of clearness, it may throw light upon the psychology of *Eindringlichkeit* or insistence.

No higher honor can be paid a scientific theory than critical discussion based upon experiments which are conceived and carried out expressly to test its validity. Here, however, is a discussion that leaves out of account the original statement of the theory, and relies wholly upon secondary sources; and here are experiments that fall into line with the work of Schumann and his school, but by the same token are directed upon a complex process of assimilation. It is a great disappointment.

[1] *Op. cit.*, 40, 42 f., 54 ff., 67 ff., 75; K. Mittenzwey, *Psych. Stud.*, II., 1907, 386 ff. ("Im Begriff der Assimilation findet sich von einem Merkmal der Klarheit zunächst gar nichts.")

Postscript.—Since writing this paper I have learned that Dr. Britz is numbered among the victims of the war. It goes against the grain to criticise thus sharply an author who can no longer reply. Yet I am sure that Britz would have wished his work to be seriously considered; and since the points really at issue are not personal, but scientific, it is perhaps not too much to hope that some other pupil of Schumann or Lipps may carry further the study of clearness which Britz began.

COMPOUND SUBSTITUTION IN BEHAVIOR

BY S. BENT RUSSELL

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The average man does not seem to care much how his brain works. There are many inquiring minds, however, that are striving to add to the sum of human knowledge of the brain and its operations which serve to adjust the man's inner relations to his outer relations. The search for facts in this field is certain to be guided more or less by theories that are based on the results of preceding investigations.

These theories, of course, must be remodeled from time to time. At the present day we find that there are certain theories of nervous mechanisms that are given some acceptance in scientific circles. In brief outline these theories show that each nervous impulse enters the nervous system at a receptor that is either external or internal. It then passes from neurone (nerve fiber) to neurone across the connecting synapses or junctions. Following the course that is most open to it, it tends to arrive at one or more muscles or other effectors. The theories give a clear account of reflex action. They also account in some degree for associative memory in its elementary forms. They show for example an explanation of substitution of one stimulus for another and an explanation of memorizing a series of words.[1]

In this article it is proposed to extend these theories and seek to explain more complex responses or rather responses to more complex stimuli such as appear in first lessons in mental arithmetic, for example. We will begin by noting some effects of greater numbers of neurones. We will then discuss briefly the formation of simple associations and the substitution of one stimulus for another through association and also the linking of movements into a definite order or series. The relation of such a movement series to what is known as

[1] Watson, John B., 'Behavior,' New York, Holt, 1914, pp. 272, 274.

delayed reaction will then be taken up. With this preparation we will attack the main problem before us; *i. e.*, responses to complex stimuli.

To make a proper beginning we will start with a brief definition of the more important terms we shall use.

Selective reaction is a form of simple association. It is the type of behavior shown when a child points to an object upon hearing its name pronounced. It is always the result of training or experience.

Substitution will be used to express simple substitution and is the type of behavior where one stimulus takes the place of another in provoking a given movement. It is the result of training as in the case of young chickens running up at the sound, 'Here chick, chick,' without waiting to see the food.

Delayed reaction is the term used for a case of selective reaction or substitution when the response does not come immediately after the determining or substitute stimulus. Several seconds or several minutes may elapse between the stimulus that determines the movement and the corresponding response.

A memorized series is a series of movements made in a definite order from previous training. When a dog is made to 'fetch' he performs a memorized series that he has been taught.

Compound substitution is the same in apparent form as simple substitution but requires the coöperation of several stimuli to provoke the response. Herbert Spencer in explaining instinct[1] used the term compound reflex action to describe automatic behavior in which complex stimuli produce complex movements. In compound substitution we have a similar term. When you teach a boy to give a correct answer to the question "Two and one make how many?" you are developing a mechanism for compound substitution. If we attempt to analyze this case after the habit is fixed, we see that the sound of the word two cannot have a great tendency to provoke the utterance of the word three. We see

[1] 'Principles of Psychology,' New York, Appleton, 1894, Vol. 1, p. 432.

that the sound of the word one cannot have a great tendency to provoke the word three. The same is true for the other words of the question asked, when taken separately and yet taken together, the words do provoke the response 'Three.' How may we account for this?

We see at once that it is much harder to account for compound substitution than it is for simple substitution. It is not so very difficult to think of connecting nervous pathways that would allow one stimulus to excite a movement which before training required a different stimulus. Neither is it very difficult to think of connecting paths that would allow a stimulus from one receptor to open up a passage for a stimulus from another receptor, as in a case of selective reaction. When we come to compound substitution, however, we find a different matter. We must now provide for coöperation of stimuli, or, if you prefer it, for the resolution of nervous impulses. It would seem that a new principle is required. This is the problem which we are to discuss. If the theories we now have covering association and substitution are worth demonstration, the extension of these theories to cover compound substitution must be also worth while.

Having this much knowledge of what we are seeking and why, let us proceed with the search. Having noted what a great gap there is between simple substitution and compound substitution, let us now hunt for the missing links.

In the course of this discussion it will be shown that the key to the mechanisms for a memorized series will prove to be a missing link as it were.

Let us here briefly consider the effect of numbers of nerve fibers in the system. The greater the number of nerve fibers that are simultaneously aroused and tributary to a given muscle the stronger will be the contraction if any occurs and moreover the greater will be the tendency to contract actually. The superiority of the human mind over the brute mind must be largely due to the greatly superior number of the neurones that control movement. A dog can be trained to respond to a simple command but not to a long speech. A child can be

trained to respond to a simple command and by further training, he comes to respond to very lengthy instructions.

The act of a thoughtful man is due to the joint excitation of many neurones. To educate a child, we first train him to respond to simple commands from the effect of association. For each act thus learned, one or more association nerve fibers must be developed. We may say that each step of learning is the development of a particular association fiber. Step by step, the child learns to talk. Step by step, he learns to write, to read, and to count.

By way of further preparing a foundation for our discussion, let us agree that selective reaction and substitution can be explained by the dual common path theory, which is that for every unit association of which the nerve system is capable, there is a common nerve path which is, we will say, an association fiber. Moreover each association fiber has two tributary fibers or private nerve paths coming from two different receptors or sensory terminals.

Each impulse that follows the common path leaves a resistance temporarily lessened for a subsequent impulse. Each impulse from a private path goes by way of the association fiber to a certain muscle or other effector. If the resistance is low enough, a feeble impulse will reach the effector. Let us call such an impulse a scout impulse when it only serves to lower the resistance for the next impulse. Let us also, on the other hand, call an impulse that follows a scout impulse and is strong enough to aid in causing movement, a worker impulse for convenience in discussion. When a volley of worker impulses excite one of a pair of antagonistic muscles, movement will take place unless there be equal excitation of the opposed muscle.

By way of illustration, let us think of a child writing under the direction of his teacher. A certain movement may be prompted by a group of worker impulses from his eyes which are guiding his hand together with a similar group from his ear due to the teacher's words and another group of tactile impulses from the feel of the penholder, etc., and still another group from his hand and arm; *i. e.*, kinæsthetic impulses from

the muscles and joints due to movements just made. All these worker impulses reinforce each other and compel the contraction of a certain muscle. This illustration shows how some of the so-called voluntary movements are caused. Such movements are determined by previous training as each worker impulse comes by way of an association fiber which has been developed by previous impulses. It is plain that the formation of a single letter is due to a large number of association nerve fibers working together like the instruments of a great orchestra.

To apply the theory of the dual common path to a typical case of selective reaction, we may use the following illustration. The sound of the word 'ball' spoken by the nurse excites a short series of scout impulses in the boy's brain, each opening up some common path. The sight of the ball produces worker impulses that follow along the common paths opened by the scout impulses. Of course these paths were developed by the repetition of impulses in some previous experience. The worker impulses now provoke the movement of pointing at the ball. The result is due to recency and frequency of stimuli in altering resistance to conduction.

In the case of substitution, the application is similar. Let us take this illustration: Show a horse an ear of corn and then call him, thus giving him a lesson. After several lessons, he will come at your call without any corn. The explanation is that the lessons opened up the dual common paths so that the call after training produces a sufficient volley of worker impulses to provoke a forward movement. To express it in a different way the effect of the lessons is to set up a 'conditioned reflex' as it is called.

In his presidential address given in the March, 1916, number of this journal Professor John B. Watson showed how the conditioned reflex has been used in behavior experiments and suggested its use for investigations of association reaction, etc. Let us here note that in the cases he described the conditioned reflex is the result of simple substitution and appears when the substitute stimulus provokes a sufficient volley of worker impulses to produce reaction. In the case

of compound substitution, however, the time element is a factor. Hence, although we find a resemblance in the operations, the term reflex may be thought inappropriate.

In order to help the reader keep in mind the way impulses tend to follow recent impulses from other receptors, the following illustration is offered: Suppose that Mr. Brown leaves his home in the suburbs in the morning after a snowstorm, on the way to the railway station. He breaks a path through the snow. A little later his neighbor Mr. Jones leaves his home and soon strikes Brown's trail. His easiest course is to follow Brown's track to the station. We may say that Brown represents a scout impulse and Jones is like a worker impulse that coming from a different source soon after the scout impulse, tends to follow the same nerve path because it offers less resistance.

Having now a conception of how elementary nerve mechanisms are constituted, let us think how they may be coördinated so as to account for a memorized series in which the movements follow the same order in which they were made before, in a similar situation. Each movement causes afferent or kinæsthetic impulses, some of which may be worker impulses that excite the next movement while others are scout impulses that open up the paths for other movements to follow. The kinæsthetic impulses have become substituted for other stimuli that were received in early training before the habit was fixed. In this way the movements become linked together so that it is only necessary to provoke the first movement of the series and if the external conditions are right, the other movements will follow automatically in their proper order.

On consideration we see that the important factors for a memorized series are the kinæsthetic impulses and the association nerve fibers leading to the muscles that must act. An important thing for us to remember in this connection is that the scout impulses sent through by one movement of the series will facilitate the worker impulses for a movement that follows after intervening movements and a considerable interval of time.

Let us now pass on to the behavior known as delayed reaction.

Let us suppose a small dog placed in a box so constructed that he can see two openings in front of him, one to the right and one to the left but cannot reach either until the bar in front of him is removed. Over each opening is a green light that can be switched on or off by the operator. One opening always leads to food, the other does not. Sometimes one opening and sometimes the other leads to food. To train the dog the operator turns on the light over the opening with food. After giving the dog time to notice the light he turns it off and half a minute later releases him. After a number of trials the dog becomes trained so that when released he goes at once to the opening where the light was seen. We may explain the behavior in this way: The sight of a light over the right-hand opening excites certain nerve paths. That is, it causes impulses that follow certain nerve paths. The performance of going to the right-hand opening is a series of movements due to impulses following certain nerve paths. Let us suppose that there are particular association nerve paths that are common to each group. That is to say, there are particular association nerve fibers that are aroused in executing the movement series that are also aroused by the sight of the right-hand light.

The common paths will get two excitations whenever the dog makes the proper turn at the proper time. The double excitation may be assumed to have the useful effect of opening these common paths; *i. e.*, of lowering their resistance. Each succeeding lesson will further facilitate the appropriate behavior. When the common paths are sufficiently developed we may explain the dog's actions by saying that the right-hand light produces scout impulses that temporarily open up further the common paths and the operation of releasing the animal produces worker impulses that follow these common paths and provoke the movement of turning to the right. It appears then that delayed reactions are explained by the dual common path theory.

We may say that delayed reaction is by nature only a

special case of selective reaction. As a further aid to understanding the matter, we may note that a case of delayed reaction is like a memorized series where some of the movements are omitted so as to leave a gap in it. For in every movement series that is habitually made, the movements in the first part of the series are associated with each movement in the last part of the series. In other words each movement causes impulses that facilitate or insure any movement that has a place further along in the series as we have already observed.

We may say then that delayed reactions are special cases of memorized series behavior. Or we may say with equal propriety that a memorized series is a compound of delayed reactions, meaning by this reactions that are more or less delayed. For each movement of such a series, to some extent, is determined by impulses that preceded it by a considerable interval of time.

Behavior students have measured the comparative intelligence of animals by the delayed reaction and by the memorized series as shown in escaping from a maze or opening a puzzle box. The more intelligent an animal is the quicker it will master a given maze or puzzle box and the longer it will remember what it has learned.

The principle of learning is no doubt the same with the animal that learns in a few lessons as with the animal that requires many. The difference must be in the nerve structure with which he begins the task. The animal of high intelligence must have more association neurones to take part in the proceedings.

From this brief study it would appear that the delayed reaction can be explained in some measure by the same mechanisms that are used for simpler forms of behavior.

We have considered four types of behavior, selective reaction, substitution, the memorized series, and delayed reaction and we find in all of them it is a matter of association nerve fibers that register the frequency and recency of impulses traversing them in the range of experience and then in turn regulate the passage of impulses that provoke movement.

To put it another way association is the characteristic feature of the four types of behavior.

This brings us to the main question, the explanation of compound substitution.

Let us consider the case of a boy learning mental arithmetic. In the beginning he is taught simple problems. He is taught to make a certain response to a certain series of ear stimulations. For example when he hears the question: "Two and one make how many?" He must respond, "Three." The reader will see that we have here a case of compound substitution. As the boy advances in the study, the series of excitations is increased in length so that there are more factors to be kept in his mind as it were. In mastering each series he really develops certain association fibers; *i. e.*, his experience opens up certain nerve paths. The associations formed in the earlier lessons take part in working out the later problems.

But let us look into the matter more closely. Suppose that the boy learns to add by counting on his fingers. At the sound of "two" he holds up two fingers. At the sound "one" he holds up one finger of the other hand. In response to "How many?" he puts his hands together and counts "one, two, three" and answers "Three." Here we have a memorized series in which eye movements, finger movements and vocal movements are linked together by association. Let us note that quite a large number of association neurones are stimulated in the course of the performance. After the boy has mastered this series, he can be trained to modify it by counting in a whisper and uttering the final word three aloud. Then he can be taught to suppress the finger movements so that they are incipient only. The eye movements also become incipient. It is fair to suppose that the number of neurones stimulated though lessened, is now nearly as great as when all the movements were actually made, so we still have a large number of neurones taking part in the response.

On consideration we see that a certain series of auditory excitations provokes a memorized series composed of move-

ments or incipient movements that lead up to the desired vocal response. In the case where the intermediate movements are only incipient, we may say that the series of excitations given by the question heard, provokes a series of scout impulses and the effect upon the *nerve muscle system* produces a group of worker impulses that excite the series of actual movements included in the proper response. These nervous operations by long practice become greatly abbreviated, but we may believe that the successive character of the operations still remains.

This shows that a case of compound substitution may be evolved from a memorized series, so we see how an elementary case of compound substitution may be explained as a modified memorized series so to speak. The same reasoning may be followed to build up cases of more and more complexity. The behavior of a skillful lawyer engaged as referee in a legal contest is largely determined by compound substitutions which are due to his special training. His ability to keep in mind an astonishing array of pros and cons has been built up step by step. Each step in the case means more movements linked in habit series and a greater number of association neurones stimulated. When the great lawyer finally gives his decision for the plaintiff or for the defendant, the deciding movement is prompted by the discharge of a large number of association neurones which have been duly prepared in the course of the proceedings.

A performing elephant will respond to his trainer's signal by one trick or another depending upon the stage setting at the time. In the same way, the great lawyer responds to his stage setting; *i. e.*, the pros and cons of the legal problem. The lawyer's response, however, is due to a far greater number of association neurones and to a far greater number of steps in the ladder of learning.

It may be noted that the above given explanation of compound substitution rests to some extent upon an assumption that all incipient movements can in some way cause afferent

impulses similar to the kinæsthetic impulses from actual movements.[1]

If our view is the correct one, a case of compound substitution may be regarded as a memorized series that has become modified by suppressing the movements so that of the different stimuli concerned, each takes part in determining the resultant response and thus the stimuli do coöperate. It is evident too that in the case of compound substitution of a long series of stimuli, delayed reaction plays an important part for the reason that it takes part in all memorized series as we have seen.

It is hardly necessary to point out that language habits are the basis of most compound substitutions in human behavior. Writing movements also are an important aid in forming the required associations.

From this demonstration we may conclude that a rational theory can be formulated for nervous mechanisms for compound substitutions in behavior if we may start with a very large number of association nerve fibers to many of which there are tributary afferent fibers coming from the muscles. These fibers are developed by practice through the influence of frequency and recency. As brooks come together to form a river so do the worker impulses meet and provoke movement. These nerve mechanisms are of great service in adapting behavior to environments containing changeable features. In the instinctive behavior of all highly organized animals they have an important part. They are especially important in child education. In fact we might almost say that in any animal that has a cerebral cortex, behavior is mainly determined by such mechanisms or at least modified by them.

If our theory is the true one, we shall find that to have a greater intelligence, the number of association fibers must be increased in much greater ratio.

If we take a broad view of the subject, compound substi-

[1] A discussion by the writer of 'The Function of Incipient Motor Processes' will be found in the PSYCHOL. REV., 1915, 22, 163–166. The other main assumption made in this article and termed the dual common path theory is discussed by the writer in the PSYCHOL. REV., 1916, 23, 235.

tution in behavior may be defined as an increase of the correspondence between the organized individual and its environment in speciality and in complexity. Furthermore it is the coördination and integration of correspondences, so to speak. The subject is such an extensive one that the demonstration given herein, to some readers, may appear quite inadequate. On the other hand, to some readers, it will seem a lengthy exposition of a simple proposition. It is not practical in a short article to do more than offer a contribution to the general study of this extensive subject with sufficient elaboration to make the main idea plain to those who are searching for mechanisms to explain mental growth.

THE DELAYED REACTION IN A CHILD

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Introduction

The present paper derives its chief significance from the fact that the child tested did not possess vocal language and probably not gesture language either.

In the work with the delayed reaction on children previously published,[1] five subjects were used: M., 8 years, Hd., L., and H., each 6 years, and F., $2\frac{1}{2}$ years. All of these children possessed vocal language. M. received 38 trials on delays; Hd., 46 trials; H., 15 trials; L., 41 trials. All of them succeeded with delays as great as 25 minutes. There was some indication that the children first encountered particular difficulty in the intervals from 4–6 secs. The child F. received 507 trials on delays. Her maximal delay was 50 secs. The periods of greatest difficulty were at 5 secs., 7 secs., 10 secs., 15 secs., 30 secs., and 40 secs. Although F. was given 45 trials on the one-minute delay and failed, it is possible that more prolonged training would have enabled her to master the interval.

When the present tests were begun, the writer hoped to secure subjects who would fill in the great gap between the child F. and the older children. Success has not attended these efforts mainly because of a lack of convenient material. One three-year-old child was tested, but the data are of so little value that they will not be presented. The second and fundamental purpose of this study was, however, to study the delayed reaction in a child too young to possess vocal language. This is of particular importance because of Watson's insistence that the delayed reaction, if solved by internal factors other than orientation, must be solved by vocal language.

[1] Hunter, Walter S., 'The Delayed Reaction in Animals and Children,' *Behav. Monog.*, 1913, 2, No. 1, pp. 52–62.

The Subject Tested.—The subject of these tests was the writer's daughter, Thayer, thirteen to sixteen months of age from first to last of the experimentation. She was a normal, healthy child physically—a little slow perhaps on the behavior side, due to the lack of the constant attention that many children receive. She learned to walk alone rather suddenly at about 15 months; and by 16 months, she could indicate with a little certainty her eyes, ears, nose and mouth. When 13 months old, she could 'throw a kiss' and wave 'bye-bye.' Even before this, as early as the tenth month, she swayed and waved her arms to music. These observations are presented as a sample of her best accomplishments. She had no vocal language. She made many sounds, some of which were in response to definite stimuli; but in no case did she use the sounds spontaneously and in no case did she use them as symbols. Her equipment was not large in the first place, and what there was was purely of a stimulus-response nature. During the period covered by the experimentation, the following vocal behavior was present: 'Dăddy' she said whenever a distant door was heard and I was away, or when she heard me coming up the steps. 'Whĭtte' came in response to the striking of the clock or the ringing of the door bell. She could say 'bo͞ob-bo͞ob' for the dog; 'dāy-dāy' for the duck; 'm-m-m' for the cow; a funny noise for the donkey; and 'y-gŏb, y-gŏb' for the turkey. These were in response to the specific questions, "What does the dog say?" etc. Some of these she got mixed up and later forgot all but the duck, the turkey, and the cow. In addition to these vocal responses there occurred only the more conventional baby noises.

One gesture might possibly be termed language, viz., raising her arms to be taken up. It is impossible to say, however, that this was not in response to present stimuli. It is also impossible to say with any certainty whether or not the child used a vocal cry specifically "to attract attention" to her needs. I have no evidence to indicate that such was the case. Experimentation was purposely stopped before the first signs of language (in the conventional sense) appeared.

Apparatus and Method.—The apparatus used is shown in Fig. 1. It consists of three boxes placed upon a stand whose top is 6 inches above the floor. Each box is approximately 3 inches deep, 4 inches wide, and 5 inches long. Each is covered by a hinged top. The apparatus was wired for electric lights, but these were never used. Another type of apparatus more nearly approximating that used with children in the previous work was constructed. It, however, proved unsuited to Thayer and was used only with the three-year-old child.

The only features necessary to secure in devising an apparatus for the delayed reaction are these: (1) It must be adapted to the size of the subject and to its mode of response—walking, reaching, swimming or flying. (2) It must provide a means for presenting a stimulus in one of several places. (3) These stimulus positions must be equally accessible to the response. And (4), the stimulus and the method employed should be such as to present no differential cues to the subject during the intervals of delay. These requirements, although rigid, are simple and can be met for practically all organisms. I am therefore unable to agree with Professor Yerkes when he says[1] that the multiple choice method of studying ideational behavior is superior to all others in: (1) applicability to a wide range of conditions; (2) susceptibility to standardization; (3) quantitative nature of results; and (4) intelligibility of data acquired. Nor can I acquiesce in the claim that "It is already obvious that the method enables us to compare, as has never before been possible, the responses to certain standard situations, of human and infra-human, normal and abnormal, mature and immature subjects." Both the multiple choice and the delayed reaction methods are valuable for the study of human and animal behavior; but they are applicable, I think, to very different problems.

In the experiments reported in this paper, the following method was used. Thayer sat in front of the apparatus, as shown in Fig. 1, and the stimulus object was placed in her

[1] Yerkes, Robt. M., 'Methods of Studying Ideational Behavior in Man and Other Animals,' PSYCHOL. BULL., 1915, 12, 330–331.

hand. A great variety of things were used for stimuli: dolls, keys, rattles, shoe-buttoners, small books, etc. Every effort was made to keep up the child's interest. Occasionally two different stimulus objects were used in the course of the day's work. Such methods are necessary if a child of this age is even to approximate to the vigor with which a hungry

FIG. 1. Thayer opening box *c*.

animal attacks its problem. The stimulus object was taken away from the child almost as soon as she received it and was placed in one of the boxes. The lid of the box was left open and Thayer was pushed over (or permitted to lean over) and made to look into the box. Often she tried to reach in and get the stimulus; but in every case, her hand was withdrawn and she was raised back to an upright position. The lid of the box was now closed. Save for a few instances to be mentioned in due time, the subject was distracted during the interval of delay. Distraction took either one of several forms: (1) I might place my hands over her eyes and rock her body back and forth from right to left. (2) She might be stood up, turned around with her back to the apparatus, kept there awhile and then put down. (3) I might cause her to turn her head by speaking to her. She would remain in this position and imitate animals for me (as described

above under vocal habits) as long as any delays here used required. About 2 secs. before the end of the delay period, her body was straightened around and she sat facing the middle box, *b*, entirely free from contact with me. (She almost never looked back at me; and when she did, she paid no further attention to the problem. I never spoke to her during the delay. These facts together with the child's inability to reach delays of a minute or more, indicate that she was not deriving cues from the experimenter.) Thayer was now left to her own devices until she opened the box containing the stimulus object. In all but a few cases, she began to hunt for the stimulus as soon as she was straightened around. Time was taken with an ordinary watch and was counted from the moment the box lid was closed until the subject made some movement toward one of the boxes. In the records particular attention was given to the orientation at the time of response and to the behavior during the delay. A reaction was counted wrong if the child opened any box save that containing the stimulus.

Experimental Results

In this experiment there was no period of learning the association between the stimulus object and the three boxes. Seeking for objects that had disappeared was already a part of the subject's behavior equipment. I noticed as early as her eleventh month that if I showed her a toy and then hid it behind something, she would immediately reach or creep toward the spot. I have no doubt, however, that this type of reaction occurred earlier. The present test was more complex than this in that the toy might be in either one of *three* different places.

I quote the following from my diary records indicating the results at the very beginning of the work. "Date, 10–23–15. Trial 1. Toys put in middle box and door shut. (All this done by Thayer on her own initiative.) I now put my hands over her eyes and shook her head and whole body playfully but thoroughly. No orientation of upper part of body retained. 13 secs. delay from time she straightened up

after closing box until she reached toward *b*. She reacted correctly, straight to middle box."

"Trial 2. Toys put in right box, *a*. 12 secs. delay with distraction as above. Reacted correctly."

"Trial 3. Same as No. 2, 13 secs. delay. O. K."

"Trial 4. Thayer preferred right or middle box. I had her put toys in left one, *c*. Distraction by standing her up and turning her to me. 17 secs. O. K."

"Trial 5. Middle box used. Baby tired of test. 14 secs. Distracted as in first trial. Reaction wrong."

In these tests made on the first day, Thayer missed one of five or 20 percent. The delays ranged from 12–17 secs. Distraction was always used. The same orientation at the moment of release was held for all, viz., orientation to *b*. These long delays were very startling and held out a promise of very rapid development through training. This was the last of October. A longer stage of delay was not successfully reached and consistently maintained until after Christmas. This fact is shown in Table I.

TABLE I

Delay in Secs.	Correct Trs.	Wrong Trs.	Percent. Correct
3	3	0	100
4	1	0	100
5	6	0	100
6	3	0	100
7	2	2	50
8	4	2	66
9	4	1	80
10	11	4	73
11	6	2	75
12	6	3	66
13	5	0	100
14	6	8	42
15	13	11	54
16	5	5	50
17	4	3	57
18	2	1	66
19	1	1	50
20	9	15	37
21	1	5	16
22	2	3	40
23	1	0	100
24	2	3	40
25	0	3	0
26	1	1	50
30	3	5	37
35	2	0	100

This table shows the size of the delays and the number of correct and incorrect reactions made. All trials given the subject through December 2 are included. The statement is not chronological. The reason the intervals of delay increase so gradually is that within a few seconds variation, Thayer herself determined when she would begin the reaction. All I could do was to place her facing the apparatus and await results. Table II. groups the delays into five

TABLE II

Delays	Percent Correct
3–7 secs.	88
8–12 "	72
13–17 "	55
18–22 "	37
23–35 "	44

classes which may be called the 5 sec., 10 sec., 15 sec., 20 sec., and 25 sec. intervals. The 10 sec. interval may be regarded as mastered, but no higher interval. (This is understated, as will be indicated below.) Table III. gives data gathered

TABLE III

Delay in Secs.	Right	Wrong	Percent Right
5	4	0	
6	0	1	77
7	3	1	
8	2	0	
9	1	0	
10	10	3	82
12	2	0	
15	25	8	75
20	24	10	70
25	2	2	50

from January 2 through January 10. An interval of one month had elapsed during which no tests were made. A comparison of Tables II. and III. indicates a marked gain in ability to deal with the 15 sec. and 20 sec. intervals. In Table III. the 15 sec. interval can be regarded as mastered and the 20 sec. interval as practically perfected.

The tables just given are valuable in showing just what the child actually did in the work *as a whole*. Her achieve-

ments are much obscured, however, by such a presentation, inasmuch as poor incentives and position factors frequently dragged her total percentages down. Here, *e. g.*, is the diary record for the ten tests made November 12. (Data included in Table I.) The delays were all above 20 secs., and 7 trials of 10 were successful. Where the word 'distracted' is used,

TABLE IV

Box with Toy	Delay		Behavior	
a	20 secs.	distracted	*a*	
a	22 "	"	*bca*	
c	21 "	"	*abc*	
c	20 "	"	*c*	
b	35 "	"	*b*	very slow and 'careful.'
a	22 "	stood her up	*a*	
c	26 "	distracted	*c*	
b	20 "	"	*b*	
c	22 "	"	*c*	
c	24 "	stood her up	*ac*	

Thayer's eyes were covered and her body was shaken back and forth; or she was induced to look up at the ceiling and listen to me count while I waved her arms about. The letters in the last column indicate to which box the reactions were made. In every case the subject was oriented, body and face, to *b* at the moment of reaction. This day's record is better than any that preceded it. Two weeks previously, she had succeeded with intervals between 11 and 19 secs. But during those two weeks that followed, she was largely the victim of position habits. The day following the above diary record, she again fell back into position habits. I kept holding pretty well to 15 and 20 sec. delays with an occasional one at 30 secs. The task was too difficult, however, and she shifted from one position habit to another.

Work was discontinued for a month. By the end of this time, the following changes had occurred in the child: (1) old position habits were temporarily lost; (2) new interest was taken in the problem; (3) greatly increased control of her own body appeared—shown mostly in walking and balancing; and (4) a stronger aversion to being held during distractions had developed.

Table III. above summarizes the results for this period. It also understates the subject's behavior. On January 5, she made the entire day—9 trials—at 15 secs. without error and with no correlation between orientation and direction of response. She now fell into a position habit, but recovered and made 20 secs. delay correctly 5 times in succession, again with no dependence upon orientation.

Thayer's best delays may be recorded as 20–24 secs. The child F. used in the earlier work reached a delay of 50 secs. F.'s record would probably have been higher had she been tested with a method similar to the one here employed, *i. e.*, a method where the satisfaction is derived from the stimulus object and not from an associated food supply. The gap between Thayer and F. would undoubtedly be bridged in a gradual manner by a continuous increase in periods of delay. Greatest interest now centers on children of less than one year of age. How early ontogenetically does this ability to react independently of orientation appear?

It remains to comment upon the position habits and errors that appeared. The frequency with which these stereotyped forms of response interfered with the work and *the fact that the child if permitted would watch the box containing the toy during the interval of delay*, indicate the great importance of kinæsthesis in the response. Position habits occurred with each of the three boxes so that during a particular position habit period Thayer always chose a particular box first. I made no tests where the choice lay between two boxes as opposed to three. Time is limited both by the speed with which a baby grows and even more by the necessity of staying within the limits of the child's interest and patience. Table V

TABLE V

					Total Reactions Made
Order of response.....	*cba*	*cab*	*ca*	*cb*	
No. made.....	17	13	8	28	66
Order of response.....	*abc*	*acb*	*ac*	*ab*	
No. made.....	6	3	2	11	22
Order of response.....	*bac*	*bca*	*bc*	*ba*	
No. made.....	7	3	7	9	26

analyses all incorrect responses and gives the relative number of times the subject followed the different possible orders. Thus when an error was made, 17 times Thayer first opened *c*, then *b* and then *a*. The table shows that three times more errors were made beginning with box *c* than with any of the others. When the subject opened *c* first, she opened *b* next 45 times out of 66, or 68 percent of the time. When she opened *a* first, she chose *b* next 15 times out of 20, or 75 percent of the time. When *b* was opened first, *a* was chosen next 16 times out of 26, or 61 percent of the time. In other words, when the reaction began at the end of the apparatus the tendency was to take the boxes in order until the solution was reached. Only six times in all did the subject go to the same box twice in the same trial. These cases are distributed throughout the entire period of experimentation. The following is a record of the order of the boxes chosen:

cccab
bcba
cacb
cacb
bcba
cacacab.

Of the 114 errors recorded in Table V., 32 (28 percent) occurred when the box containing the toy on the last previous trial was re-selected. Inasmuch, however, as such a mode of response often led to success the percentage is very low. This form of behavior as well as that of the six instances above given is apparently far less current in the present subject than in Hamilton's dog.[1] The later study made by Hamilton[2] reports the case of a child 26 months old. Out of 38 trials, 60.53 percent (34.21 plus 26.32) of the reactions involved the type of behavior given just above as occurring but 6 times during the present work, 264 trials. Since Thayer missed 120 trials (66 plus 22 plus 26 plus 6), her percentage is 5.

[1] Hamilton, G. V. T., 'An Experimental Study of an Unusual Type of Reaction in a Dog,' *J. of Comp. Neur. Psychol.*, 1907, 17, 329–341.

[2] Hamilton, G. V. T., 'A Study of Trial and Error Reactions in Mammals,' *J. of Animal Behav.*, 1911, 1, p. 51.

This extreme difference in behavior is undoubtedly due to one or both of the following causes: (1) the guiding influence of the absent stimulus in the delayed reaction tests; and (2) the fact that only three boxes were used here as opposed to Hamilton's four. It would be very interesting to determine whether a variation in the number of boxes would result in a corresponding variation in 'reaction tendencies.' If this were true, the possibility of phyletic correlations would be pushed still farther back than appears in Hamilton's work.

It will be valuable to put beside this work, similar data gathered on rats and raccoons in 1910–1912. The records here given are representative and include only tests made with three boxes on periods of delay. The following indicates the maximal delays attained by the animals whose records are used in this paper:

Rat No. 9, maximal delay 10 secs.
Rat No. 2, " " 1 "
Dog Blackie, " " 5 mins.
Raccoon Bob, " " 30–35 secs.[1]

Table VI. summarizes the errors made by these four animals. It includes for comparative purposes the data for Thayer. The raccoon's records include delays from 1 sec. through 20 secs.; those for the dog, from 1 sec. through 7 secs.;

TABLE VI

Animal	No. of Trs.	Total No. of Errors *A*	3 Place Errors *B*	Persistent Errors *C*	Percent. of *A* to *C*	Percent. of *B* to *C*
Thayer	264	120	54	6	5	11
Raccoon, Bob	720	209	78	29	13	37
Dog, Blackie	570	127	75	25	19	33
Rat No. 9	575	144	42	13	9	30
Rat No. 2	345	152	69	47	32	60

those for rat No. 9, from the third stage of delay (turning light off just as animal was released) through 7 secs.; and those for rat No. 2, from the third stage of delay through 1 sec. I have included the data represented by 'percent of *B* to *C*' because Hamilton's percentages are based only on those reactions that included all the boxes of his apparatus.

[1] These data are taken from my 'Delayed Reaction,' pp. 35–38.

The column '3 place errors' includes the trials that involved a testing by the animal of each of the three boxes. By 'persistent errors,' I mean all errors that involve trying any one box more than once each trial. These were all 3 place errors. This column corresponds to reactions belonging to Hamilton's types *D* and *E*.

The only one of Hamilton's human subjects whose percentage in *D* plus *E* rose above 6.45 percent was a 26-months-old child whose grade was 60.53 percent. Of the animals below man, the lowest grade (best record) was 22.58 percent made by a dog. Hamilton's results and my own here presented indicate a marked difference between man and other animals in reactive tendencies, *i. e.*, in forms of kinæsthetic habits. (There is, I think, no clear evidence as yet that the tendencies are instinctive.) Whether this is caused by phyletic factors, or by experimental and environmental conditions is a matter undecided. My infra-human animals are essentially on a par. And so I think are Hamilton's in that practically all of them made their highest percentages in what I here term 'persistent errors.' (His curves would be quite different, naturally, if *D* and *E* were combined and if *B* and *C* were combined.[1]) Much work is undoubtedly needed to determine how minute a classification of reaction tendencies can be and still be significant for animal ability.

Theoretical Considerations

There is very little in the way of interpretative comments that I can add to what has already been said in other papers.[2] The delayed reaction problem can be solved at least in two ways:[3] (1) by the maintenance of bodily orientation in whole

[1] Hamilton, *op. cit.*, 1911, p. 54.

[2] 'Delayed Reaction,' pp. 62–79 and Hunter, W. S., 'A Reply to Some Criticisms of the Delayed Reaction,' *J. of Phil. Psychol., &c.*, 1915, **12**, 38–41. See also Watson J. B., 'Behavior,' 1914, pp. 224–227; and Ch. X.

[3] If there is a present determining external stimulus, the reaction is not delayed. A. C. Walton, 'The Influence of Diverting Stimuli during Delayed Reaction in Dogs,' *J. Animal Behav.*, 1915, **5**, 259–291, has shown that dogs can react successfully to three boxes after delays of 30 secs. when they have been distracted during the intervals. This is better than I had been able to show. Before deciding that the dog belongs in a class with the raccoons—as perhaps he does—it will be necessary to

or in part during the interval of delay or by the chance recovery of the proper orientation just at the moment of release; and (2) by the use of some intra-organic factor which is *non*-observable by the experimenter. In the first method, the animal always responds in accordance with orientation; in the second, he does not. The cue used in the second method may or may not be retained in the focus of neural activity during the delay. It is highly improbable that such retention occurs under conditions of distraction. What one has, then, is a system of processes or cues which 'stand for' certain differential responses as a result of association. These cues are susceptible to selective re-arousal and subsequent successful functioning in initiating responses. This is the condition which I have previously found in raccoons and in one child, F., 2½ years old. It is the condition here presented by Thayer, ages 13–16 months. This second method of solution which I am describing may be mediated by any type of intra-organic process which can be re-aroused without the presence of the external stimulus, toys as used in the present study. Inasmuch as kinæsthetic factors can be so aroused,[1] and inasmuch as they have been demonstrated to have great importance not only in animal reactions in general but in the delayed reaction in particular (position habits and maintenance of orientation), it is most probable that the intra-organic factors are kinæsthetic in nature. (The genetic relations of sensation, image and imageless thought are discussed in 'The Delayed Reaction,' pages cited.) In certain cases this type of process has its locus in the vocal organs with frequent resulting audible sounds. This we term vocal language as it occurs in normal human adults and in children of a certain development. In other cases the observance of the sounds and their accompanying behavior

have data on the animals' orientations at the moment of response. This Walton unfortunately does not give. The only facts that we have are that the animals did not maintain their orientations during the delays. If it should appear that the animal is able to recover the proper orientation in a large number of instances after a thorough distraction and can then react correctly, this fact will itself be of great significance and will require careful analysis.

[1] See the all too brief comments in my review of Calkins' 'First Book in Psychology,' PSYCHOL. BULL., 1915, 12, 189–190.

does not indicate that the organism uses either the sound or the parallel kinæsthesis as a substitute of the type above described. This is the situation in all animals that indulge in vocalization. But this intra-organic kinæsthetic factor may arise elsewhere than from the throat. Some part of the general bodily musculature may be the origin. Here, when the behavior is overt, we speak of gesture language. When it is not overt, the delayed reaction method has proved serviceable in detecting it. Language is ideational in function. So also are the cues which function in many responses of raccoons, of children and possibly of dogs (Walton). The resulting conception of these cues is that they are kinæsthetic sensory ideas. This line of reasoning leads one to conclude that a true language non-vocal in character appears phylogenetically and ontogenetically prior to vocal language. Such language, although undoubtedly of great service to the individual in controlling his reactions, is of little social significance.